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National  
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## STRATEGY FOR OUTER PLANETS EXPLORATION

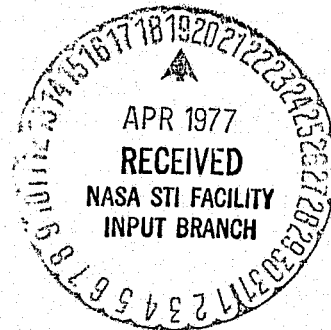
(NASA-TM-X-74634) STRATEGY FOR OUTER  
PLANETS EXPLORATION (National Aeronautics  
and Space Administration) 94 p HC A05/MF  
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# **STRATEGY FOR OUTER PLANETS EXPLORATION**

**June 11, 1975**

**National Aeronautics and Space Administration**

**Washington, D. C. 20546**

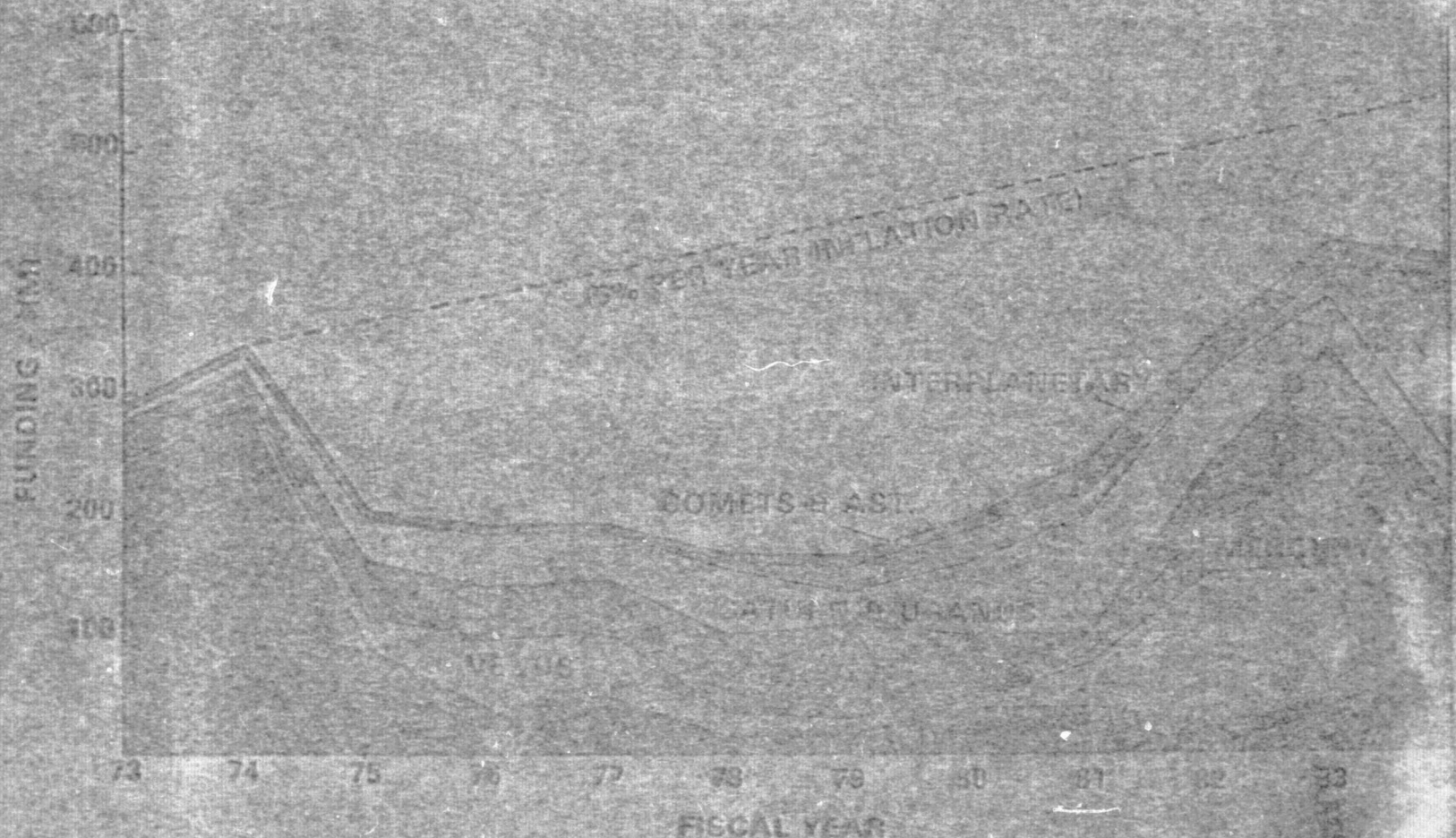
ISSUES: PLANETARY PROGRAM

1. MERITS OF OSS PLANETARY MISSION MODEL? (SPECIFIES MARINER JUPITER/URANUS MISSION IN 1979, MARINER COMET ENCKE MISSION IN 1980 WITH PHASE B EFFORT IN FY 1977, AND PJOP MISSION IN 1980 OR 1981.)
2. RELATIVE IMPORTANCE OF MARINER-CLASS MISSION TO URANUS (REQUIRES JUPITER/URANUS OPPORTUNITY IN 1979 AND FY 1977 NEW START)?
3. WHAT IS THE PROPER LEVEL OF ACTIVITY FOR PLANETARY EXPLORATION IN THE POST-VIKING ERA?
4. SHOULD NASA MAINTAIN THE TITAN/CENTAUR CAPABILITY UNTIL EQUIVALENT SHUTTLE/IUS IS OPERATIONAL?
5. ARE SINGLE LAUNCHES ACCEPTABLE FOR ANY OR ALL PLANETARY MISSIONS? ACCEPTABLE WITH BACKUP OPTION?





# PLANETARY EXPLORATION FUNDING DISTRIBUTION (2/18/75 MISSION MODEL)

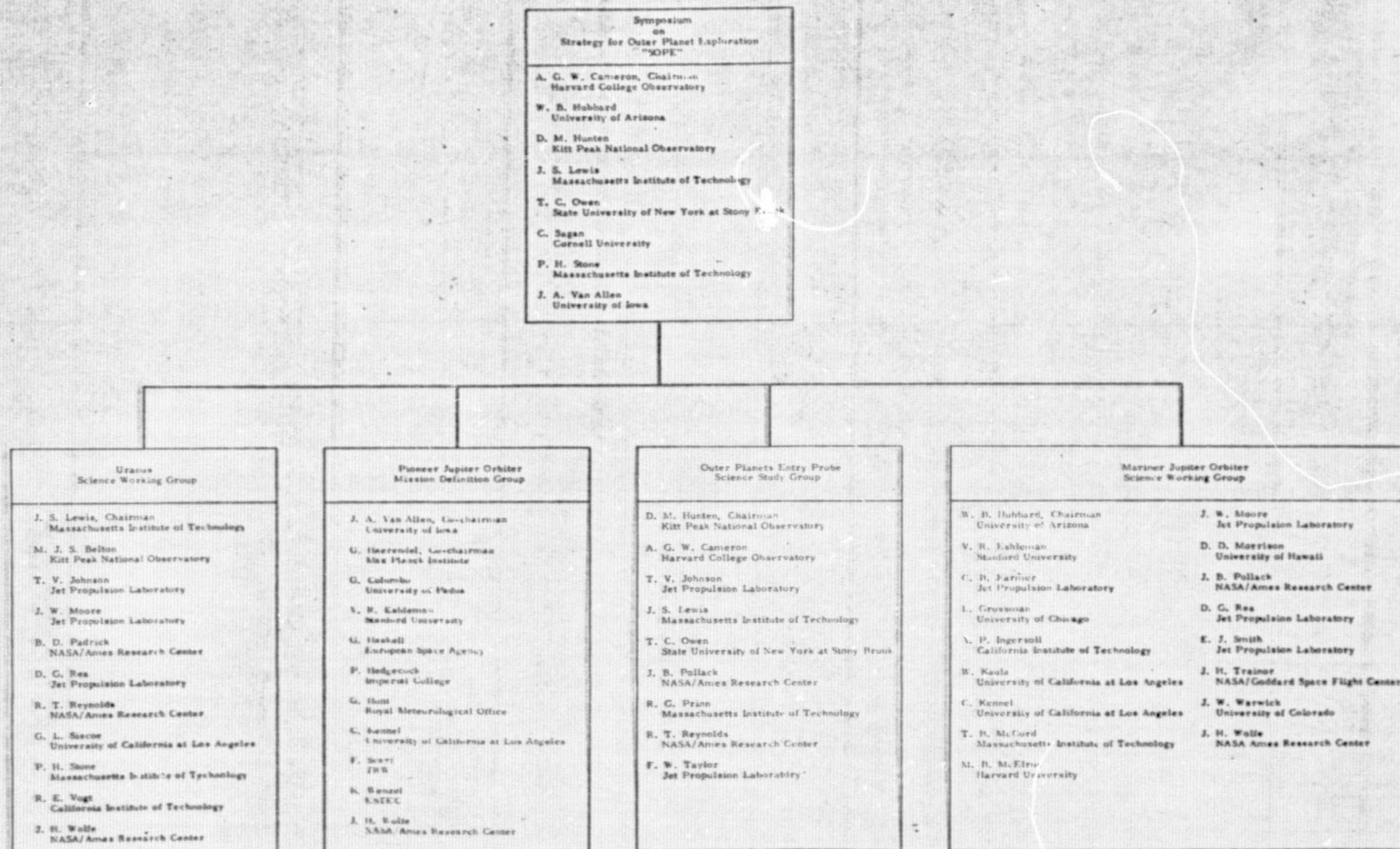


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# FORMULATION OF OUTER PLANET STRATEGY



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## FINDINGS

### SYMPOSIUM ON STRATEGY FOR OUTER PLANET EXPLORATION

- 1.1 "JUPITER SYSTEM INTENSIVE AND URANUS SYSTEM EXPLORATORY MISSIONS ARE OF GREAT AND COMPARABLE SCIENTIFIC IMPORTANCE; SUCH MISSIONS ARE TECHNICALLY FEASIBLE IN THE NEAR TERM (1979-1981 LAUNCHES)."
- 1.3 "MISSIONS DESIGNED TO GO TO THE ORBIT OF URANUS AND BEYOND ARE ESSENTIAL TO THE INVESTIGATION OF THE INTERPLANETARY AND INTERSTELLAR MEDIA."
- 1.5 "FOR URANUS MISSIONS:
  - a. OF HIGHEST SCIENTIFIC IMPORTANCE ARE AN ENTRY PROBE INVESTIGATION OF THE URANIAN ATMOSPHERE, GOOD IMAGING OF THE SATELLITES, AND PARTICLES AND FIELDS MEASUREMENTS. OF VERY GREAT IMPORTANCE IS INFRARED SPECTROSCOPY AND RADIOMETRY.
  - b. THE MOST HIGHLY DESIRABLE MISSION IN CONCEPT IS MJup, WHICH SHOULD BE INSTRUMENTED IN SUCH A WAY AS TO GIVE GOOD MEASUREMENTS IN PURSUIT OF THE ABOVE OBJECTIVES.
  - c. IF IT IS NOT POSSIBLE TO CARRY OUT AN MJup MISSION, THEN THE SCIENTIFIC OBJECTIVES OF THIS MISSION MUST BE DIVIDED BETWEEN TWO MISSIONS, MJU AND A PIONEER PROBE CARRIER TO URANUS, EACH OF WHICH IS AN EXCELLENT MISSION COMPLEMENTARY TO THE OTHER. IN THIS EVENT, SOPE BELIEVES THAT MJU SHOULD BE THE FIRST MISSION, TO BE LAUNCHED IN 1979 FOR CELESTIAL MECHANICS REASONS, AND SOPE FURTHER BELIEVES THAT A PIONEER CARRYING A PROBE TO THE ATMOSPHERE OF URANUS SHOULD FOLLOW AS SOON AS POSSIBLE THEREAFTER. THE MJU MISSION WOULD ALSO MAKE AN IMPORTANT INCREMENTAL ADDITION TO THE STUDY OF THE JOVIAN SYSTEM."

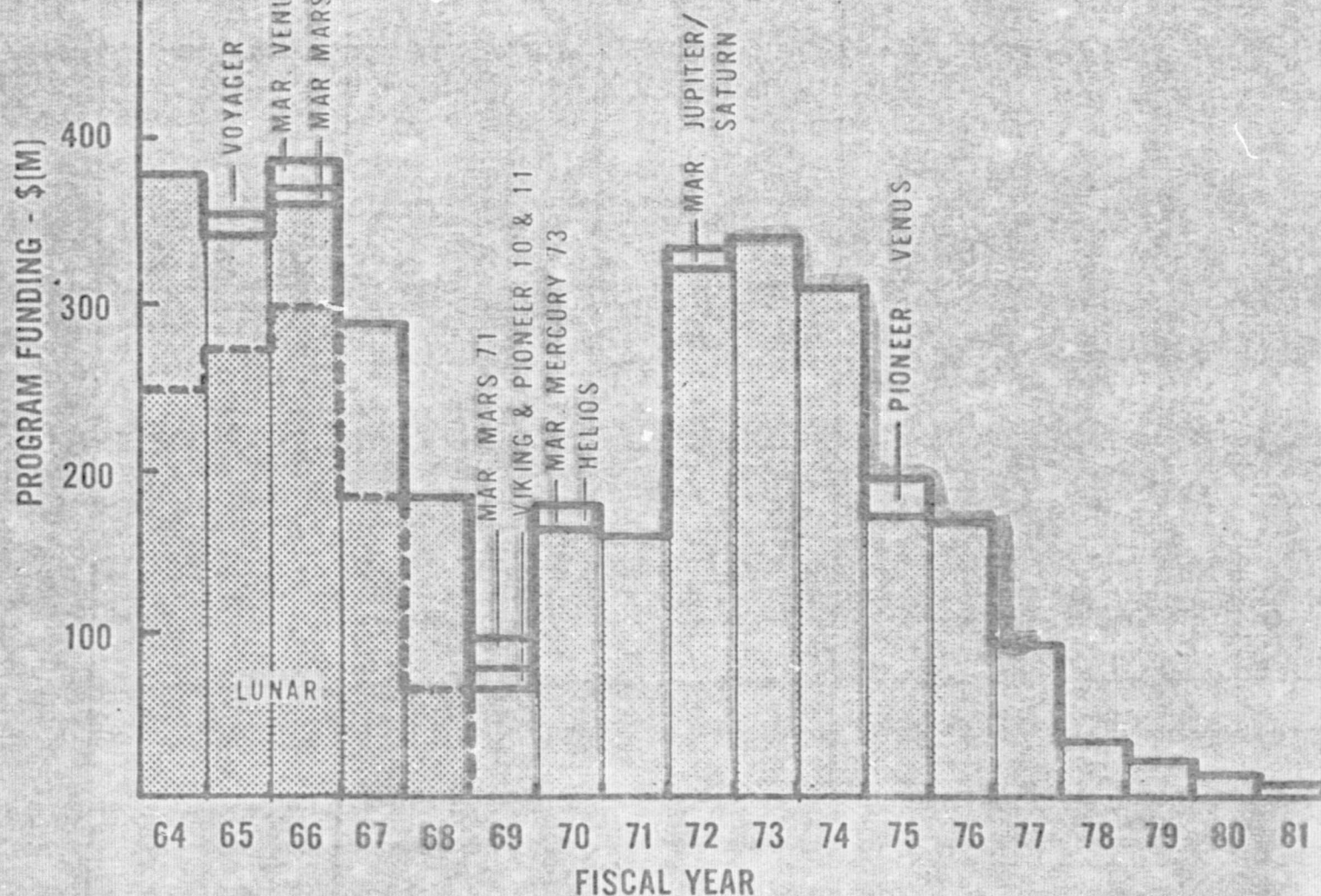
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TOP VERTICAL



# HISTORICAL FUNDING SUMMARY PLANETARY FLIGHT MISSIONS

Funding in FY75 Dollars



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(FY 75 \$M)

	<u>FY80</u>	<u>TRANS</u>	<u>77</u>	<u>78</u>	<u>79</u>	<u>80</u>	<u>81</u>	<u>82</u>	<u>83</u>	<u>84</u>	<u>85</u>	<u>TOTAL</u>
MARINER SUBSYSTEM BUY	5.4	1.3										6.7
MJU PROJECT			13.4	29.2	25.7	7.2	7.3	5.5	5.4	7.1	8.1	<u>108.9</u>
									TOTAL			115.6
T/C LAUNCH VEHICLE	.4	.4	8.0	20.6	9.9	2.9						42.2
BACK-UP LAUNCH VEH.	(.2)	(.2)	(1.8)	(17.8)	(14.7)	(3.0)						
												<u>157.8</u>
									TOTAL (INCLUDING L/V)			157.8

## PREFACE

One of the recommendations from the Space Science Board study, "Opportunities and Choices in Space Science, 1974," was "that NASA undertake an immediate reexamination of the strategy for exploration of the outer solar system during the next decade." In response to that recommendation, NASA's Planetary Programs Office formed a number of scientific working groups to study in depth the potential scientific return from the various candidate missions to the outer solar system. (See accompanying chart.) The results of these working group studies were then brought together in a series of symposia to evaluate the potential outer planet missions and to discuss strategies for exploration of the outer solar system that were consistent with fiscal constraints and with anticipated spacecraft and launch vehicle capabilities. The participants in the symposia, listed below, included the chairmen of each of the working groups, augmented by additional experts in planetary sciences:

A. G. W. Cameron, Chairman  
Harvard College Observatory

W. B. Hubbard  
University of Arizona

D. M. Hunten  
Kitt Peak National Observatory

J. S. Lewis  
Massachusetts Institute of Technology

T. C. Owen  
State University of New York at Stony Brook



C. Sagan  
Cornell University

P. H. Stone  
Massachusetts Institute of Technology

J. A. Van Allen  
University of Iowa

This document is a distillation of the numerous technical and programmatic discussions during the symposia, and it describes what the participants concluded to be a logical, scientifically sound and cost effective approach to exploration of the outer solar system.

In addition to the participants in the symposia, I wish to thank all the members of the working groups and the personnel from NASA Headquarters, the Ames Research Center, the Jet Propulsion Laboratory, and Science Application, Inc., who provided detailed descriptions of the various candidate missions and performed various analyses in support of the working group deliberations. To all of these contributors go our sincere thanks for a most thorough analysis of complex issues and for clear, well-thought-out and concise conclusions.



Robert S. Kraemer  
Director of Planetary Programs  
National Aeronautics and Space Administration

FOLDOUT FRAME

# FORMULATION OF OUTER PLANETS

Symposium on Strategy for Outer Planet Exploration "SOPE"		Symposium on Strategy for Outer Planet Exploration "SOPE"
A. G. W. Cameron, Chairman Harvard College Observatory		for Outer "
W. B. Hubbard University of Arizona		er on, CH College Obs
D. M. Hunten Kitt Peak National Observatory		ed of Arizon
J. S. Lewis Massachusetts Institute of Technology		ational O
T. C. Owen State University of New York at Stony Brook		etts Instit
C. Sagan Cornell University		ersity of N
P. H. Stone Massachusetts Institute of Technology		iversity
J. A. Van Allen University of Iowa		etts Instit
		en of Iowa

Uranus Science Working Group
J. S. Lewis, Chairman Massachusetts Institute of Technology
M. J. S. Belton Kitt Peak National Observatory
T. V. Johnson Jet Propulsion Laboratory
J. W. Moore Jet Propulsion Laboratory
B. D. Padrick NASA/Ames Research Center
D. G. Rea Jet Propulsion Laboratory
R. T. Reynolds NASA/Ames Research Center
G. L. Siscoe University of California at Los Angeles
P. H. Stone Massachusetts Institute of Technology
R. E. Vogt California Institute of Technology
J. H. Wolfe NASA/Ames Research Center

Pioneer Jupiter Orbiter Mission Definition Group
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G. Haerendel, Co-chairman Max Planck Institute
G. Colombo University of Padua
V. R. Eshleman Stanford University
G. Haskell European Space Agency
P. Hedgecock Imperial College
G. Hunt Royal Meteorological Office
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Outer Planet Science Study Group	
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A. G. W. Cameron Harvard College Observatory	A.
T. V. Johnson Jet Propulsion Laboratory	A.
J. S. Lewis Massachusetts Institute of Technology	T.
T. C. Owen State University of New York at Stony Brook	J.
J. B. Pollack NASA/Ames Research Center	T.
R. G. Prinn Massachusetts Institute of Technology	J.
R. T. Reynolds NASA/Ames Research Center	R.
F. W. Taylor Jet Propulsion Laboratory	R.
	F.

# OUTER PLANET STRATEGY

Symposium on Outer Planet Exploration "SOPE"
Chairman Harvard College Observatory
University of Arizona
National Observatory
Massachusetts Institute of Technology
University of New York at Stony Brook
University
Massachusetts Institute of Technology
University of Iowa

WOLDOUT FRAME 2

Outer Planets Entry Probe Science Study Group
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Mariner Jupiter Orbiter Science Working Group	
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C. B. Farmer Jet Propulsion Laboratory	J. B. Pollack NASA/Ames Research Center
L. Grossman University of Chicago	D. G. Rea Jet Propulsion Laboratory
A. P. Ingersoll California Institute of Technology	E. J. Smith Jet Propulsion Laboratory
W. Kaula University of California at Los Angeles	J. H. Trainor NASA/Goddard Space Flight Center
C. Kennel University of California at Los Angeles	J. W. Warwick University of Colorado
T. B. McCord Massachusetts Institute of Technology	J. H. Wolfe NASA Ames Research Center
M. B. McElroy Harvard University	

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## SECTION 1

### FINDINGS

- 1.1 Jupiter system intensive and Uranus system exploratory missions are of great and comparable scientific importance; such missions are technically feasible in the near term (1979 - 1981 launches).
- 1.2 An orderly program of atmospheric entry probes is essential for the investigation of the atmospheres of Jupiter, Saturn, Titan, Uranus, and Neptune. The quantities which should be determined include atmospheric structure, atomic and molecular abundances of major and minor constituents, cloud properties, and net radiative flux.
- 1.3 Missions designed to go to the orbit of Uranus and beyond are essential to the investigation of the interplanetary and interstellar media.
- 1.4 In the middle term (1982 - 1990 launches), follow-on investigations of Saturn, Titan, and the remainder of the Saturnian system will be highly desirable. Investigations of the other outer planets and their satellites should follow in a logical progression from near term missions.
- 1.5 For Uranus missions:
  - a. Of highest scientific importance are an entry probe investigation of the Uranian atmosphere, good imaging of the satellites, and particles and fields measurements. Of very great importance is infrared spectroscopy and radiometry.
  - b. The most highly desirable mission in concept is MJUp, which should be instrumented in such a way as to give good measurements in pursuit of the above objectives.
  - c. If it is not possible to carry out an MJUp mission, then the scientific objectives of this mission must be divided between two missions, MJU and a Pioneer probe carrier to Uranus, each of which is an excellent mission complementary to the other. In



this event, SOPE believes that MJU should be the first mission, to be launched in 1979 for celestial mechanics reasons, and SOPE further believes that a Pioneer carrying a probe to the atmosphere of Uranus should follow as soon as possible thereafter. The MJU mission would also make an important incremental addition to the study of the Jovian system.

1.6 For Jupiter orbiting missions:

- a. For various practical reasons, Jupiter orbiting missions must be divided into at least two classes, each scientifically complementary to the other. The PJOp mission is dedicated to atmospheric entry probe delivery and to the spatial and temporal mapping of the outer Jovian magnetosphere. The MJO mission is satellite-intensive and gives good synoptic coverage of the Jovian atmosphere. SOPE considers it very important that both of these missions be performed.
- b. The Pioneer Jupiter Orbiter will be a powerful platform for exploring the whole magnetosphere, especially its outer parts. With a probe it can also make unambiguous measurements of Jupiter's atmosphere, and the mission will initiate a whole program of probing in the outer solar system. SOPE therefore believes that PJOp should be flown first. The probe would be more valuable if inserted into the light side of Jupiter rather than the dark side, thus requiring a Type II trajectory to Jupiter.
- c. The MJO mission will be enhanced if additional scientific capability can be developed beyond the MJS capability, which can respond to discoveries made during the MJS missions.
- d. SOPE is dismayed at the prospect that it may not be possible to launch an MJO mission between 1981 and 1987. SOPE believes it to be essential that NASA provide the launch capability to place a Mariner spacecraft in Jupiter orbit in any launch year (so that MJO may respond to MJS in a timely fashion).

- e. SOPE believes that MJS mission planning should take into account measurements that will be done well in Jupiter orbiting missions, and arrange scientific compromises accordingly. In particular, SOPE believes that in its reassessment of the flight trajectories, the MJS project should consider the importance of improved phase angle coverage for the imaging of the Galilean satellites, in order to determine whether more coverage of at least one satellite at about 90° phase angle is feasible.

1.7 Continued earth-based astrophysical investigations of the outer planets and their satellites supported by appropriate laboratory studies are essential components of an overall strategy to explore the outer solar system.

## SECTION 2

### DISCUSSION

#### 2.1 Background

The exploration of the outer planets of the solar system began with the flyby of Jupiter by Pioneer 10, and has continued with the flyby by Pioneer 11. The next step in the exploration of the outer planets will be the Mariner missions to Jupiter and Saturn (MJS), to be launched in 1977, with a flyby of Jupiter followed by subsequent flybys of Saturn in late 1980 and 1981. Meanwhile, there is a chance that Pioneer 11 will remain a viable spacecraft and will provide some information about Saturn in 1979.

The basic task of the Seminar on the Strategy for Outer Planets Exploration has been to determine an optimum strategy for the further exploration of the outer solar system, knowing that severe fiscal constraints will limit the possible activity in this area. The strategic possibilities for consideration by the Seminar were sharpened through the activity of several NASA committees and working groups.

During 1973-74 the MJU Science Advisory Committee recommended that advantage be taken of the unique 1979 launch opportunity to send a Mariner mission to Uranus carrying an atmospheric entry probe. When this recommendation was presented to the Committee on Planetary and Lunar Exploration (COMPLEX) of the Space Science Board, it appeared that fiscal constraints would not allow the full MJU mission with probe to be flown; therefore, COMPLEX recommended that an MJU mission without probe be launched in 1979.

Meanwhile a joint NASA-ESRO Committee had been studying a Pioneer Jupiter Orbiter mission with probe, which might possibly use the components of the Pioneer H spacecraft. Also, preliminary studies at JPL had been done on a possible Mariner orbiter mission for Jupiter. This mission would be relatively costly, owing to the extended operations cost for a two or three year mission in orbit about

Jupiter, during which time a satellite is likely to be encountered about once a month. The Committee on Planetary and Lunar Exploration, although recognizing the great scientific attractiveness of the MJO mission, recognized that a 1981 mission could not be fitted into the funding profile because of an apparent conflict with the funding for the Large Space Telescope. Therefore, COMPLEX recommended that the MJO mission be flown in the middle 1980's, and that the PJO mission with probe (PJO<sub>p</sub>) be launched in 1980.

Subsequently, the Space Science Board considered all of the missions recommended by its committees, and gave very high priority to the Large Space Telescope. The funding profile which had been given to the Space Science Board for its consideration failed to accommodate all of the missions recommended by its committees by approximately \$200,000,000 spread throughout the late 1970's and early 1980's. The SSB concluded that, within the constraints of this fiscal exercise, it would probably not be possible to do two expensive missions to the outer planets during this time period, but it was unable to make a choice between the MJU mission and the PJO<sub>p</sub> mission. It therefore recommended that further consideration be given to the desired strategy for outer planets exploration.

In response to these developments the structure of the committees within NASA studying various aspects of outer planets exploration was modified. The MJU Science Advisory Committee was transformed into a Uranus Science Working Group which also studied Pioneer missions to Uranus. The PJO Committee had produced a final set of recommendations, and no further PJO mission studies were performed. The Outer Planets Entry Probe Science Study Group considered the technical problems associated with putting probes into the atmospheres of all of the outer planets. An MJO Science Working Group was established, and is currently active. These studies have provided valuable inputs to the Seminar on the Strategy for Outer Planets Exploration.

Meanwhile, technical studies of the major missions proposed for the next few years have modified the funding profiles which were

considered by the SSB. Both the concept of and the funding profile for the Large Space Telescope have been undergoing continuing modification. The cost estimates for Mariner missions to the outer planets have been scaled down, through the adoption of single rather than dual launches, and other economies. Thus it is no longer clear that fiscal constraints will preclude a flyby mission to Uranus launched in 1979 and an orbiting mission to Jupiter launched in 1980 or 1981.

## 2.2 General Considerations

One consequence of the fiscal scenario considered by the SSB is that a choice might have to be made between a major mission to Uranus and a Jupiter orbiter. It is not presently clear that this choice must be made, for an early mission to each planet may be possible.

A mission to Uranus will be exploratory, whereas an orbiting mission to Jupiter will be Jupiter-intensive, designed to answer specific questions posed by the results of previous missions. In a balanced program, both exploration and intensive investigation have a role to play, but should one have both in a time of severe fiscal constraint?

Discussion at SOPE has tended to show that any one Jupiter-intensive orbiting mission tends to favor just a few scientific disciplines. Since many scientific disciplines have a strong stake in planetary research, their interests tend to be served better by a balanced program.

This is why SOPE believes that a balanced program is the wisest choice. The practical application of such a policy would be for NASA to try very hard to obtain new starts for a Uranus mission in FY77 and for a Jupiter orbiting mission in FY78 or FY79.

## 2.3 Uranus Considerations

Uranus is a planet of potentially very great scientific interest. It is an example of a new class of planet hitherto unexplored; its bulk composition may be considered to be basically cometary in nature, with a relatively small mass fraction of hydrogen and helium added.

This is in contrast to Jupiter and Saturn, where the hydrogen and helium constitute the bulk of the planet. An especially interesting aspect of Uranus is the large tilt of its spin axis, some  $98^\circ$  with respect to the normal to its orbit plane. The orbit plane of the regular satellites is similarly tilted, appearing to lie in the equatorial plane of the planet. The production of a system like this is an extremely interesting cosmogonic problem. It will be particularly interesting to see whether Uranus has a substantial magnetic field and associated magnetosphere, and if so, then a new class of magnetospheric phenomena may be observed during the middle 1980's, when Uranus is approximately pole-on toward the sun. Opinions are greatly varied as to whether the methane to hydrogen ratio is or is not greatly enhanced above the solar value, and the rotational period of the planet should be regarded as uncertain by a factor of two.

Uranus possesses a regular system of satellites situated in a plane roughly perpendicular to the approach trajectory of a mission to Uranus launched in 1979. These satellites appear to have radii of only a few hundred kilometers, and great interest attaches to the question of whether they have significant compositional and morphological differences with respect to satellites in the Jovian and Saturnian systems. A Mariner spacecraft flying by Uranus could obtain good images ( $<1$  km resolution) of Miranda, the innermost satellite, and fair images of the remaining regular satellites, which form a fairly compact system. The CCD line-scan imaging system being developed for Pioneer spacecraft would provide a single frame with a few kilometers resolution on Miranda and a small number of additional images of the planet and other satellites at useful resolutions.

The Uranus Science Working Group remains of the opinion that an MJU mission with probe is the optimum scientific mission to study Uranus, but it recognizes that lower cost missions must be studied in view of current fiscal constraints.

The optimum launch opportunity remains in 1979, and involves a Jupiter swingby. A less attractive Jupiter swingby opportunity occurs in 1980.

A Pioneer mission launched without a probe in 1979 can have a four year flight time to Uranus with a Jupiter swingby at about  $2R_J$  but this probably involves an unacceptable radiation hazard. If the Jupiter swingby occurs at  $5R_J$ , the trip time becomes 5.3 years. USAC considers a mission of this type to be the minimum mission which would return good scientific results, but SOPE believes that it is not competitive with other good missions owing to the relatively low resolution of the imaging and the lack of an opportunity to probe the atmosphere.

If the Pioneer spacecraft carries a probe for Uranus atmospheric entry, then the trip time remains 5.3 years. This trajectory allows retargeting of the spacecraft to Neptune should this prove desirable.

In the middle 1980's it will be possible to launch Pioneer spacecraft missions to Uranus which involve Saturn swingbys. This is the basis for the so-called PSUp series of missions, in which the Pioneer spacecraft would carry probes that could enter into the atmospheres of Saturn or Uranus, for which a common probe design is possible, and of Titan if the probe does not need redesign for that atmosphere. In the middle 1980's, a spacecraft delivering a probe to Saturn could not be retargeted to flyby Uranus. It may also be possible, at significantly increased cost, to send Mariner missions to Uranus in the middle and late 1980's. These would be direct flights to Uranus either with the assistance of Solar Electric Propulsion or through the use of an Expendable Tug from a shuttle launch. "VEGA" trajectories, involving flybys of Venus and Earth before a direct flight to Uranus, are also possible, but these appear to be undesirable because of a nine year flight time and because of the required additional spacecraft environmental control associated with the Venus swingby. Thus, although it is possible to contemplate Mariner follow-on missions to Uranus in the



late 1980's, it must be recognized that these would be considerably more costly than utilizing the 1979 Jupiter swingby opportunity.

SOPE considers that there are three scientific areas of greatest importance: planet and satellite imaging; investigation of the Uranian atmosphere with an entry probe; and particles and fields measurements to determine whether Uranus has a magnetosphere and trapped radiation, as well as to explore the interplanetary medium to larger distances from the sun and probably beyond the heliopause into the interstellar medium.

Only the Mariner spacecraft can carry imaging cameras able to obtain ~one kilometer or better resolution of the planet and the satellites. It is very desirable to use CCD imaging because of the greater sensitivity to the red end of the spectrum, in view of the low light levels at Uranus. The CCD line-scan imaging system being developed for Pioneer spacecraft would give useful results on the planet, but less useful results on the satellites, and the number of images would be severely limited by data transmission rates.

The radio occultation technique has so far given ambiguous results for the Jovian atmospheric structure. It appears that use of occultation techniques for outer planet investigations will be useful, but it will demand very accurate measurements and will contain interpretive instabilities. Therefore, this technique by itself cannot be trusted to give accurate structures of outer planet atmospheres. Hence atmospheric entry probes are essential to determine the structure of the Uranus atmosphere. Probes can also make in situ measurements of atmospheric composition more reliably than can remote sensing techniques. Either a Mariner or a Pioneer can carry a probe. One slight drawback of a probe-carrying mission is that it may not be possible to fly by the surface of the planet close enough ( $\sim 1R_U$ ) to measure the  $J_4$  gravitational moment of Uranus.

If Uranus has a magnetosphere, the magnetic pole will face much more directly into the solar wind (for a 1979 launch) than is the case for any

other planetary magnetosphere. This is a unique opportunity for particles and fields measurements, and many unusual phenomena can be anticipated. Although the heliopause marking the boundary between the interplanetary and interstellar media may be between the orbits of Jupiter and Saturn, it is more likely to be between the orbits of Saturn and Uranus. Thus only for a mission to Uranus is good operational coverage of the mission through the expected heliopause assured by spacecraft design and communications scheduling. A Pioneer spacecraft facilitates measurement of pitch angle distributions, but this is outweighed by importance by the better data storage and communications rates for a Mariner spacecraft, which allows better retention of measured data during the cruise phase beyond Uranus.

Another area of great scientific importance is infrared radiometry of the planet. This is important for determining the overall heat balance of the planet and pole-to-pole differences in effective radiating temperature. This would be nicely complemented by an upwards-and-downwards measurement of infrared fluxes at a single point by an entry probe. Infrared radiometric measurements can be made from a Mariner. There is some concern about the quality of infrared radiometric measurements from a Pioneer because of the low duty cycle on a very cold planet.

It is obvious that the mission that responds best to these objectives is MJUp. It is also the most costly mission. Part of the concern about cost arises from the uniqueness of the 1979 launch opportunity; a 1980 MJUp mission would require a 10 year flight time to Uranus. Thus a conservative strategy would require preparation of a second spacecraft and launch vehicle to be ready in 1979 in case of launch failure of the first spacecraft. SOPE considers MJUp to be conceptually much the best mission.

If a less costly mission is required, then either best quality imaging or the entry probe must be sacrificed, and the candidate missions are MJU and PJUp. SOPE prefers MJU for the following reasons. An

MJU mission can make a greater contribution to Jupiter system science during its flyby than a PJUp mission. A follow-on probe mission is possible in the middle 1980's, but no further Mariner mission is possible until 1985 or later, and even then only with high energy stages whose development by that time is not now assured. The Mariner mission is also better for particles and fields measurements because of better data storage and communication rates.

However, if an MJU mission is launched in 1979, it is very important to supplement it by a PSUp mission as soon thereafter as possible.

#### 2.4 Jupiter Considerations

In these considerations we shall be mostly concerned with orbiting missions of Jupiter. At the present time the design of these missions is profoundly influenced by concern for the radiation hazard which would be posed by the orbital capture maneuver and by repeated orbits close to the planetary surface. The radiation hazard, particularly as posed by protons close to the planetary surface, is known to be particularly severe. Much more research needs to be done on the radiation hardening of materials and components and/or the shielding of instrumentation.

At the present time there is much concern for radiation protection on the MJS mission, and the testing program under way for this mission will be very useful for the technical design of future missions.

The relative emphasis to be given to various types of observations on Jupiter orbiting missions cannot fail to take into account the expected results of the MJS imaging experiments. The resolution to be expected for some images of the Galilean satellites approaches the imaging obtained of Mercury on the Mariner 10 mission. This will be a major step forward in the exploration of the Jovian system, and the manner in which Jovian orbiting missions can improve upon the scientific returns of MJS must be a prime consideration.

Table I shows the coverage of the Galilean satellites that would be obtained at different resolutions for two potential MJS mission profiles. The JSI mission (Io-intensive) gives excellent resolution over much of Io, moderately good resolution over a reasonable part of Europa, and fair resolution over parts of the other satellites. The JSG mission (Ganymede - intensive) gives excellent resolution over much of Ganymede, good resolution over much of Callisto, and fair resolution over parts of Io and Europa. This will be a major step forward in understanding four new bodies comparable in size to the moon or Mercury. However, it must be noted that much of this coverage is obtained for small phase angles which will show albedo variations but lack contrast. It may require a significant amount of time to analyze and digest this information and to decide on the major scientific questions to be asked in following missions, particularly since a similar wealth of data may be expected from the Saturn system from the same missions.

Pioneer missions are ideally suited for magnetospheric mapping, since the spinning of the spacecraft permits a better determination of the pitch angle distributions of the energetic particles. A major goal of such magnetospheric mapping should be to determine the conditions in the magnetospheric tail. Pioneer missions are also well suited to the delivery of atmospheric entry probes. Thus it is clear that a Jovian Pioneer orbiter cannot simply be considered as a scaled-down version of a Mariner orbiter, but it is of great intrinsic value in itself.

The prime advantage of a Mariner mission is good imaging. Hence it can be expected that a Mariner mission would concentrate upon satellite imaging, with 40 to 80 orbits being obtained in a three year operating period, and with a close satellite passage on each orbit. This should provide excellent images of the satellite surfaces under a variety of sun angles, and also allows a search for satellite variability, such as snows upon Io. During the cruise parts of the orbits when a satellite is not close, it will be possible to concentrate upon the

Table I. Resolution (in projected area) that would be achieved on Galilean satellites from two proposed MJS mission profiles (after R. Strom).

Satellite	JSI Mission Coverage (%)	Resolution (km)	JSG Mission Coverage (%)	Resolution (km)
Io	100	$\leq 18$	60	$\leq 40$
	75	$\leq 9$	30	$\leq 25$
	40	$< 3$	15	13
	small	0.6		
Europa	100	87-50	100	$\leq 68$
	50	$\leq 37$	40	$\leq 37$
	20	4	20	20
Ganymede	100	$\leq 120$	100	$\leq 150$
	35	33	75	$\leq 50$
	12	27	40	$\leq 4$
			small	0.9
Callisto	100	$\leq 300$	100	$\leq 150$
	50	$\leq 150$	45	$\leq 4$
	30	$\leq 50$	small	2
	15	23		

synoptic imaging of the Jovian atmosphere, thereby investigating fundamental questions concerning Jovian meteorology.

The PJO Mission Definition Group has recommended that the Pioneer spacecraft carry a probe for insertion into the Jovian atmosphere. This means that a thrust must be applied to capture the spacecraft into Jovian orbit while it is close to the planet, nominally at  $1.8R_J$ . This causes the probe to be inserted on the night side of the planet. The periapsis of the orbit would then be raised to about  $15R_J$ , and it is not envisaged that subsequent orbits would come closer than this to Jupiter. A nominal mission of three years would envisage about 40 or more orbits in which the spacecraft orbit would be pumped in

and out and turned so that the orbit traces out 4 petals of a flower. This would allow large orbital excursions toward the dawn and dusk sides of the magnetosphere, and toward the tail and bow shock regions. Finally, the orbit can be cranked up to an inclination of about  $30^\circ$  to measure the characteristics of the magnetosphere out of the orbital plane. More recent planning suggests that it would be sufficient to trace out three petals of the flower, omitting the bow shock region.

It appears that some further options deserve study. One of these would involve a longer flight time (200 or more additional days in a Type II trajectory) to Jupiter which would provide a different approach angle, so that the probe might be inserted into the daylight portion of the atmosphere. For the orbits with the larger periaipse distances, it appears that the electron fluences per orbit are of the order of 20% of those which would be received for orbits which come close to Io. Thus the total electron fluence which would be experienced by the spacecraft would not be raised by more than a moderate factor if a few of the orbits involved a lowering of the periaipse distance to the vicinity of Io. Such modified missions deserve study, for they might allow greater flexibility in carrying out the magnetosphere mapping.

Detailed scientific questions have yet to be studied in relation to the MJO mission. For this mission it is highly desirable that some close passages be made to the inner two Galilean satellites for imaging purposes, and possibly also to investigate conditions in the magnetospheric wakes. Also deserving of study is the utilization of new remote sensing techniques for geochemical and geophysical mapping of the satellites.

The best launch year for Jupiter orbital missions is 1981. Orbital constraints are not serious for Pioneer missions, and the PJO Mission Definition Group studied a 1980 launch. Minimum energy launch conditions are more critical for Mariner missions. 1981 is an easy launch year, but 1983 seems rather marginal using conventional launch vehicles with Io insertion. Launches in later years will require the development of new propulsion technologies. One scientific

strategy would be to launch the PJO mission with probe in 1980, and to launch the MJO mission in 1981, so that there would be an overlap between the two missions, allowing simultaneous measurements of particles and fields in the Jovian magnetosphere. However, it is highly unlikely that this combination of missions will be possible in the present fiscal environment.

Another issue which deserves serious study is the scientific management of an MJO mission. One of the strongest arguments in favor of the MJO mission is the potentiality for adaptability, which will allow the choice of later orbits to reflect the scientific findings of the early ones. The magnitude of the science management required to effect a good adaptive mode of operations is much greater than that involved in any previous planetary missions, and hence it is necessary to carefully scrutinize the conventional methods for management of mission operations and for selection of teams of scientific experimenters.

Following the anticipated scientific returns from the Jupiter system in 1979 from the MJS missions, it will become possible to pose important scientific questions which will require further imaging of selected targets on Jupiter and the Galilean satellites. This is the rationale usually given for launching an MJO mission in 1980 or 1981. However, if, in accordance with the SOPE finding discussed above, an MJUp or MJU mission is launched in 1979, it can be expected that good imaging of many selected targets in the Jupiter system can be incorporated in the mission profile for the flyby through that system.

This would lessen the urgency for an MJO mission. It would also increase the desirability that the MJO mission, when it comes, should have scientific instrumentation improved beyond the level of MJS or MJU instrumentation, so that second-order scientific questions can be addressed in the MJO mission. SOPE feels that upgraded scientific capabilities are highly desirable in an MJO mission, even though this may delay the mission. However, such a policy would make sense only if significant SR & T funds were to become available for instrument development.



Meanwhile, the magnetosphere measurements made by Pioneer 10 revealed many fascinating new phenomena and led to the formulation of a new concept: the "magnetodisk". But the Pioneer 11 trajectory, having a different kind of latitude dependence, showed that the magnetodisk concept needed considerable modification: the Jovian magnetosphere is thick as well as disk-like. We are not likely to learn many qualitatively new aspects of the Jovian magnetosphere from the MJS and MJU missions, because the trajectories have much the same characteristics as those already flown. Thus there is a definite urgency for a magnetospheric mapping mission; the two Pioneer missions have done a good job in laying the scientific foundation upon which such a mission can be planned.

These are the reasons why SOPE considers a PJOp mission to have the highest priority for the first Jupiter orbiting mission. Such a mission is ideally suited to carry out magnetospheric mapping, and it is a suitable vehicle to carry an entry probe for the Jovian atmosphere. Such a mission could be launched in any (13-month) year from 1980 onwards, although 1981 is best from a launch energy standpoint. SOPE considers it highly desirable to launch a PJOp mission in 1980 or 1981.

## 2.5 Saturn Considerations

The future exploration of the Saturn system is not currently under study by any NASA committee. The first investigation of the Saturn system is likely to be the flyby of Pioneer 11 in 1979, assuming a five-year viability of this mission following the Jupiter flyby. It will be scientifically very attractive to target Pioneer 11 close to Saturn, inside the rings, for magnetospheric investigations and to measure gravitational moments of the planet. This will be highly complementary to the MJS flyby mission, which will not penetrate very close to the planet.

The MJS spacecraft can be expected to arrive in the Saturn system in late 1980 and 1981. These spacecraft will give good images of the

satellites in the Saturn system, as well as of Saturn itself. One of them is intended to be Titan intensive, involving an occultation by the Titan atmosphere. A variety of investigations will be carried out on the ring particles. It is not clear whether these investigations will be easy to interpret since present planning involves very idealized assumptions concerning the character of the ring particles.

There are good launch opportunities for direct flights to Saturn in the middle 1980's. There are no problems concerning Pioneer missions, but it appears difficult to get more than bare minimum Mariners into Saturn orbit.

One class of missions which has been considered are the PSUp missions. These are Pioneer missions which can either carry probes directly to Saturn, or can use Saturn swingbys to carry probes to Uranus. This would take advantage of the potentiality for common designs for atmospheric entry probes into Saturn and Uranus. However, the Pioneer spacecraft can deliver a probe into Saturn and then fly on to Uranus only for launches in 1979 and 1980. In subsequent (and more probable) years the swingby distances for flights to Uranus are too great to allow the delivery of a probe into Saturn. Therefore the missions would have to be dedicated to one planet or the other, but this choice could be made en route. In principle the same types of missions might also be used to deliver a probe to Titan, but Titan probes may need a somewhat different design, with the ability to investigate the possibly complex organic chemistry of the Titan atmosphere. At the present time it is not even clear whether the Titan atmosphere is thick enough to allow an atmospheric entry probe of the currently envisioned designs. This point may not be resolved until the occultation of the MJS spacecraft by the Titan atmosphere in 1981.

Because all but Titan of the Saturn satellites are of relatively low mass, it is clear that a Saturn orbital mission cannot have the very great flexibility associated with Jupiter orbital missions. Saturn orbital missions would center on Titan swingbys, but they would be

expected to give excellent images of all of the regular Saturn satellites.

SOPE expects that a great wealth of new data on the Saturn system will follow from the 1979-1981 flyby spacecraft. Further Saturn-intensive missions will then most probably become desirable, but it would be desirable to wait until the data are received and interpreted before designing additional missions for the Saturn system, particularly orbiters. However, such missions will become desirable in the middle 1980's, particularly missions carrying atmospheric entry probes.

## 2.6 Entry Probe Considerations

It is likely that the first opportunity to deliver an atmospheric entry probe will be either to Jupiter or Uranus. The Outer Planets Entry Probe SSG has considered the technology for instrumentation on such entry probes. The SSG has expressed itself in favor of a Uranus entry probe for a first experiment of this type, on the grounds that the Uranus atmosphere appears to be more scientifically interesting, and because the entry conditions are more relaxed than at Jupiter. However, the scientific choice may be perturbed by possible quarantine restrictions. The Space Science Board has approved a quarantine statement for Jupiter and Saturn which is fairly relaxed, on the grounds that any terrestrial organisms introduced into the atmospheres of these planets will fairly soon be convected down to lower levels where the temperatures are high enough to assure their destruction. This issue remains open for Uranus, where the atmosphere is more quiescent, and there is so far no SSB recommendation concerning quarantine precautions for this planet. This indicates that a study needs to be made of the possible requirements for sterilization of a Uranus probe.

A Jupiter probe will require considerably more heat shield than a Saturn or Uranus probe, but the entry conditions for the Jupiter probe now appear compatible with technological constraints. A rather shallow entry angle is required, with a correspondingly narrow aiming error, but it appears that the ephemeris of the Jovian system is now

or shortly will be well enough known so that these requirements can be met. One feature of the preliminary mission design for PJOp was that the probe entered on the night side of the Jovian terminator. The Jupiter probe mission would clearly be scientifically more desirable if entry occurred on the daylight side; this requires a Type II trajectory which, in turn, necessitates 200 or more days of additional trip time.

One of the reasons why the Uranus entry probe appears scientifically somewhat more attractive is the expectation that the minor constituents in the atmosphere, such as methane, should be more abundant relative to hydrogen and helium than will be the case for Jupiter. However, scientific results would be obtained much sooner from a Jupiter entry probe. It should be recalled that Uranus entry probes can be launched through the early 1980's by means of PSUp missions.

The PJOp mission would deliver a probe with a spacecraft swingby distance of  $1.8R_J$ . This is adequate for good communication between the spacecraft and the probe. In principle it would also be possible to deliver a probe to Jupiter on a PJU mission launched in 1979. However, this would involve a Jupiter swingby of the spacecraft at  $3.3R_J$ , and this renders communication with the probe very difficult if not impossible with present technology.

At the present time the planning for outer planets atmospheric entry probe measurements is largely based on the instruments being developed for Pioneer Venus. It appears that these designs will allow good missions to be flown for entry probes for outer planet atmospheres. However, there are many unique problems associated with outer planets atmospheres, such as the large ratios of hydrogen and helium to other constituents, and it appears very desirable that additional instrument development take place for early probes.

SOPE considers that entry probes are essential to study the structure and composition of outer planet atmospheres. Remote sensing is not competitive for obtaining the same kinds of information, but rather is complementary to entry probes in certain respects. Entry probes

provide information that remote observations cannot possibly acquire, including the equivalent of "ground truth" necessary to calibrate the validity of the remote sensing observations. They permit access to lower regions of atmospheres inaccessible to remote sensing techniques. They are essential for determining accurate compositions and complex inorganic and organic chemistry. A proper comparative study of outer planets atmospheres will require entry probes at least for Jupiter, Saturn, and Uranus. Titan is likely to become a most interesting object after the MJS missions, so that a Titan atmosphere entry probe, possibly of special design, is most probably highly desirable in the middle term.

## 2.7 The Role of Earth-Based Observations

The importance of remote astrophysical investigations of the outer planets and their satellites may quickly be appreciated from the realization that our present interest in these objects is based almost entirely on the results of such studies. It should also be evident that the instrumental developments of the last few years have added considerably to the power of the basic techniques that can be brought to bear. New, large telescopes at excellent sites are becoming operational, a large improvement in the capabilities of airborne observations has just been achieved, an orbiting ultraviolet telescope will soon be launched and one may continue to expect results from smaller rocket and satellite programs.

It is not surprising, therefore, that each of the last few years has produced some fascinating new information about the outer solar system, despite the large number of observations that have been carried out in the past. Examples include the detection of ethane, acetylene, phosphine, HD, and water vapor on Jupiter, ethane and ethylene on Saturn, the thermal inversion on Titan and the relatively high temperature in the upper atmosphere of Neptune. One may anticipate detection of additional minor constituents in the atmospheres of all of these bodies, and improved models of atmospheric structure resulting from new temperature and pressure measurements. The atmospheres of

Titan, Uranus and Neptune should be tractable to a first order analysis, i. e., one should be able to develop models that are tied to observations that provide direct measurements of pressure and temperature with concomitant determinations of atmospheric composition.

Aside from their intrinsic interest, such results are important for the vital role they play in planning space missions to the outer solar system. It would be an indefensible use of our slender resources to make measurements from a spacecraft that could be obtained from Earth. But perhaps an even more basic issue is posed by our inability to design proper experiments if we do not even have the most basic information about the planets we would like to study.

It is therefore essential that continued support and encouragement be given to a comprehensive program of Earth-based observations. To be most productive, such a program must be supported by appropriate laboratory studies. As an example, the analysis of the composition of the atmospheres of Uranus, Neptune, and Titan is presently hampered by a lack of laboratory data on the absorption by long optical paths of methane. Another general problem is the lack of band strengths for appropriate bands of many of the minor constituents that are being discovered. Reflectivities of various candidates for Jovian chromophores are also wanting, and there are many other requirements of this type. There will be even more as the wavelength range over which the observations are being made is extended. Further studies on the laboratory simulation of chemical and physical processes in the atmospheres and surfaces of the outer planets and their satellites should be encouraged.

## APPENDIX A

### REPORT OF THE URANUS SCIENCE WORKING GROUP

#### 1. Introduction

The Uranus Science Working Group (USWG) was formed in November 1974 following the Space Science Board Summer Study. The principal tasks of this committee were to evaluate all feasible mission options for the exploratory investigation of Uranus in the late 1970's and early 1980's, to develop the scientific rationale and the expected science return, and to recommend a program for the initial exploration of Uranus. Building on the information base developed by the previous Mariner Jupiter/Uranus Science Advisory Committee and maintaining some commonality of membership, the USWG completed its tasks. The USWG findings are documented in this report.

#### 2. Uranus Science Rationale

##### 2.1 General

The areas of fundamental scientific interest in Uranus exploration and the associated types of data required are as follows:

##### 2.1.1 Particles and Fields

Uranus presents a unique opportunity to study solar wind interaction with a pole-on planetary magnetosphere with its associated trapped particle physics. In 1985 (the encounter date for a spacecraft launched in 1979) the rotation axis of Uranus will be only 7 degrees from the Uranus-Sun line. In addition, a mission to Uranus will permit interplanetary cruise measurements to a heliocentric distance of 20AU. Nightside imaging of Uranus could see the "footprint" of the entire magnetosphere at one time, should the brightness of the auroral precipitation zones be visible.

### 2.1.2 Atmosphere

The unique axial tilt of Uranus (and the pole-on planetary geometry obtainable from a 1979 launch) will permit observation of atmospheric motions under circumstances unavailable elsewhere in the Solar System. The cyclic alternation of strong solar heating of the north polar regions, equator, and south polar regions makes the resultant atmospheric motions and regional variations in the heat balance of great interest. The planetary rotation rate is uncertain, and the very existence of an internal heat source is undecided by Earth-based thermal infrared data. Thus, the thermal structure of the atmosphere is far less rigorously defined by current observations than is the case for either Jupiter or Saturn. The apparent weakness of atmospheric heating implies a turbulent regime different in character from those on previously explored planets. Finally, the atmospheric composition is a subject of energetic debate, with the abundances of heavy elements relative to hydrogen being as low as  $\sim 10^{-3}$  to as high as 1. In order to resolve many of these questions, an atmospheric entry probe is required. Also, investigation of planetary heat balance, regional variations in thermal emission, temperature sounding, cloud vertical structure and motions, and measurements of the integrated Bond albedo by a flyby are essential. Such measurements complement in situ measurements of local thermal structure and atmospheric composition by a probe.

### 2.1.3 Satellites

Even such basic data as the radii, masses, densities and albedoes of the satellites of Uranus are unknown. In addition to these quantities, the figures, surface morphologies, surface compositions, and thermal properties of these satellites are valuable for comparative purposes. Thus, good-quality imaging and IR spectroscopy of the satellites and precise tracking of the spacecraft are required. The desired precision in tracking is easily achieved, due in part to the compactness of the Uranus satellite system, which permits any close flyby of the planet to come quite close to all five known satellites.



#### 2.1.4 Cosmogony

The desire to unravel the peculiarities of the cosmogony of the Uranus system focuses on the bulk composition of the planet and its atmosphere, the composition of and compositional differences between the satellites, and the rate of rotation of the planet. All these matters have been addressed above.

#### 2.1.5 Interior

The most urgent requirements are for determination of the rotation rate and the lower terms in the spherical harmonic expansion of the planet's gravitational field. Any close flyby can measure J2 very well, but measurement of J4 to useful precision ( $\pm 10$  percent) requires grazing the planet, only  $0.1 R_U$  from the edge of the visible disk.

#### 2.1.6 Jupiter Science

Although the science rationale developed by the USWG has focused on the exploration of Uranus, important scientific observations of Jupiter can be made by a spacecraft launched to Uranus in 1979. Observations of the Jovian system, complementary to those expected from MJS77, will extend our knowledge of Jupiter and the Galilean satellites and their interactions with the magnetosphere. Imaging and IR observations of Jupiter and its satellites, and the ability to adjust spacecraft arrival time for a close encounter with one or more satellites (e.g., Callisto) will enhance significantly the overall science return.

In summary, the major areas of investigation of Uranus and its system are particles and fields, imaging, IR observations and in situ studies of the atmosphere. It is now appropriate to discuss these scientific requirements in the light of Mariner and Pioneer spacecraft capability, available instrumentation for flybys and entry probes, and launch opportunities in the 1979-1985 time frame.

### 3. Pioneer And Mariner Capabilities

#### 3.1 General

In the following discussion it is assumed that, for budgetary reasons, future Pioneer and Mariner spacecraft are direct derivatives of existing outer-planets spacecraft. High-cost and high-risk options, such as a despun instrument platform on a Pioneer or a spinning platform on a Mariner, are ignored.

Because of severely limited data rates for the present S-band transmitter (on Pioneers 10 and 11) beyond 10AU, it is assumed and required that an X-band transmitter be incorporated in any Uranus-bound Pioneer mission. Other modifications, such as that to the CCS to incorporate a return-to-Sun mode, are of much less impact to the present discussion.

The fundamental differences between the Pioneer and Mariner spacecraft of most importance to us are the 8-X higher bit rate (assuming X-band) achievable from the Mariner communications system and the basic differences between a spinning and 3-axis-stabilized spacecraft.

With respect to particles and field measurements, we recognize the inherent advantages of spinning spacecraft for studying spatially anisotropic fields and particle fluxes, such as the determination of pitch angle distributions of trapped magnetospheric particles. On the other hand, we regard an exploratory penetration of the bow shock and magnetosphere of Uranus to be of great intrinsic importance even without pitch-angle data. Furthermore, the far higher data rate available from a Mariner permits a great augmentation of return from cruise science. Because of the commitment to track a Uranus flyby to at least 20AU, study of the interplanetary medium is a very valuable ancillary benefit from such a mission. We expect that the spacecraft will function well past Uranus, possibly penetrating the heliopause and carrying out in situ measurements of the interstellar medium. We see clear advantages to both Mariner and Pioneer spacecraft as means to effect an early investigation of Uranian particles and fields.

The atmospheric circulation problem, satellite radii, figures, and surface morphology, and viewing of the darkside auroral zone all place important constraints on imaging system performance. Here an important distinction between Mariner and Pioneer capability emerges. An imaging system derived in part from MJS program hardware, based on a 3-axis stabilized spacecraft, has an inherent resolution capability of better than 1 km per pixel on Uranus and Miranda (assuming a flyby anywhere inside Miranda's orbit), with resolution no worse than 10 km per pixel on the most distant satellites. In addition, this capability will provide almost continuous global coverage of the development of atmospheric (cloud) dynamics on Uranus over several tens of planetary rotations with useful spatial resolution ( $\approx 500$  km/pixel). This performance also fully meets the exploratory science requirements for Uranus and satellite imaging. Further, the integration times possible on the nightside, using a framing camera as on the 3-axis stabilized Mariner, should permit imaging of the auroral zone during solar occultation.

By contrast, the best Pioneer imaging system presently foreseeable (a 160-element CCD detector line-scan system), gives poorer resolution by a factor of 6.6 for broad spectral band imaging (the factor becomes larger if the highly desirable narrow-band filters are used). This reduction in resolution has two important consequences:

1. For a periapsis radius of  $3.5R_U$ , the best spatial resolution of the satellites is degraded to 5 km per pixel on Miranda, and only 35 and 50 km per pixel on U4 and U5, respectively.
2. The duration of the "observatory phase" for monitoring the development of atmospheric phenomena on Uranus is reduced to four or five revolutions (two or three Earth days) at the most.

Also, because of the difference in real-time spacecraft data rates, the Mariner can return at least eight times as many frames as Pioneer, thus providing far more extensive coverage during the encounter phase.

Also, the severe limitation on dwell time imposed by the rotation of the Pioneer spacecraft makes nightside imaging of auroral zone activity impossible.

Infrared observations also suffer seriously from use of a rotating spacecraft, since long integration times are highly desirable when observing objects as cold as Uranus and its satellites. It appears that even global heat-balance determinations would be difficult for a Pioneer spacecraft, while satellite IR studies (including nightside thermal measurements of the Galilean satellites during the Jupiter swingby) and regional thermal flux variation measurements and temperature sounding on Uranus would be possible from a 3-axis stabilized platform.

As previously indicated, the science requirements for tracking the flyby in order to determine the J2 and J4 gravitational harmonics of Uranus and the masses of the satellites can be met in principle by a flyby spacecraft. There is, however, some question whether the ephemeris of Uranus will be adequately known to permit targeting close enough to Uranus ( $1.1 R_U$ ) to determine J4. Improved optical navigation by the Mariner spacecraft can provide the capability to target this close.

Finally, there is great interest in the atmospheric composition and thermal structure which can only be resolved by use of an atmospheric entry probe capable of measuring temperature, pressure, local heat balance, and atmospheric composition down to a pressure of about 20 bars. Important data on atmospheric structure can be learned from accelerometers and a nephelometer (if feasible on Uranus). A mass spectrometer is essential for atmospheric analysis, and a dedicated  $H_2:He$  ratio instrument or simple gas chromatograph may also be required. For purposes of comparative study of the Jovian planets and large-scale composition trends and fractionation processes in the Solar System, atmospheric entry probes are absolutely essential.

Assuming availability of the Titan III Centaur launch vehicle with an appropriate kick stage, it is possible to identify a number of specific launch opportunities for Uranus missions involving Mariner and Pioneer flybys with or without entry probes. The combination of Mariner-plus-probe is limited by payload weight to only one launch window, 1979 (the MJUp79 mission). Without the weight of the probe, Mariner launches in 1979 and 1980 (MJU79, MJU80) are possible. The lighter Pioneer spacecraft could carry a probe in either year (PJUp79, PJUp80), and Pioneer flybys without probes can proceed via Jupiter swingby in 1979 and 80 (PJU79, PJU80) or via Saturn swingby in 1981-5 (PSU81, etc.). In addition, the PSU missions can carry probes (PSU;81, etc.).

#### 4. Pioneer and Mariner Spacecraft Instrumentation

USWG has addressed the question of payload instrumentation for both Pioneer and Mariner spacecraft, and is confident that the scientific requirements for Uranus exploratory investigations can in each case be met either by existing instruments or by relatively straight-forward modifications of such instruments. We do not elect to recommend a single instrument list, but rather indicate our specific science requirements and indicate general classes of instrumentation and feasible types of measurements which can meet these requirements.

For a more complete discussion of Uranian science requirements, the reader is referred to the special issue of Icarus written by the members of the MJU Science Advisory Committee, chaired by Dr. James A. Van Allen.

Table A-1 summarizes the science capabilities of Mariner and Pioneer Uranus flybys. The entries in the table are not intended to correspond one-to-one with particular instruments, although such correspondences are in some cases clearly evident: magnetic fields are measured by magnetometers, etc.

Table A-1. Science capabilities of Mariner and Pioneer Uranus flybys

Science Area	Mariner Capability	Pioneer Capability
1. Particles and Fields	<ul style="list-style-type: none"> <li>• Magnetic field measurements; proton and electron plasma studies; plasma waves; energetic particles; cosmic rays</li> </ul>	<ul style="list-style-type: none"> <li>• Same except better geometry for particles and field studies but much lower data rate</li> </ul>
2. Atmosphere	<ul style="list-style-type: none"> <li>• High spatial resolution imaging in visible and near IR; long time-base coverage of motions;</li> <li>• Visible, IR, and UV occultation spectrometry</li> <li>• IR spectroscopic and thermal emission studies with spatial resolution</li> <li>• Photometric and polarimetric studies;</li> <li>• Dual-frequency radio occultation</li> </ul>	<ul style="list-style-type: none"> <li>• Lower spatial resolution imaging with shorter time base at given resolution and far fewer frames at encounter</li> <li>• Same</li> <li>• Difficult radiometry; whole-disk at best. No high-resolution spectroscopy</li> <li>• Same</li> <li>• Same</li> </ul>
3. Satellites	<ul style="list-style-type: none"> <li>• High resolution imaging; radius and figure determinations; surface morphology studies</li> <li>• Mass determinations (and density calculations) from spacecraft tracking;</li> </ul>	<ul style="list-style-type: none"> <li>• Lower resolution imaging; poor for figures and surface morphology</li> <li>• Same</li> </ul>

Table A-1. Science capabilities of Mariner and Pioneer Uranus flybys (continued)

Science Area	Mariner Capability	Pioneer Capability
4. Cosmogony	<ul style="list-style-type: none"> <li>● Surface composition and thermal properties; compositional mapping</li> <li>● Objectives contained in 2 and 3 above</li> </ul>	<ul style="list-style-type: none"> <li>● Degraded thermal and spectroscopic capabilities as above</li> <li>● See above</li> </ul>
5. Interior	<ul style="list-style-type: none"> <li>● Spherical harmonics of Uranus gravitational field (J2 and J4)</li> </ul>	<ul style="list-style-type: none"> <li>● Determination of J4 not possible due to projected navigation capability</li> </ul>
6. Jupiter System Science	<ul style="list-style-type: none"> <li>● IR imaging of Jupiter</li> <li>● Nightside radiometry of satellites;</li> <li>● Intensive imaging of a selected satellite;</li> <li>● IR spectroscopy of Jupiter and satellites</li> </ul>	<ul style="list-style-type: none"> <li>● Degraded imaging, radiometry, and spectroscopy</li> </ul>

## 5. USWG Findings

### 5.1 Finding Number 1

The great importance of high-resolution imaging, infrared radiometry and spectroscopy, and in situ investigations of the Uranus atmosphere lead us to favor the MJUp 79 mission as the best option available.

### 5.2 Finding Number 2

If, for reasons of instrument feasibility or fiscal stringency, the MJUp 79 mission cannot be flown, then it is clearly essential that both a Mariner flyby and atmospheric entry probe be flown as early as possible.

The launch capability of available boosters for launch windows from 1979 to 1985, together with the favorable planetary axis orientation, leads us to the conclusion that the best option would then be an MJU79 flyby without probe, followed in the early 1980s by a probe mission, such as PSUp. These two missions are both of very great scientific value, and are highly complementary.

### 5.3 Finding Number 3

USAC will endorse targeting of a Uranus flyby to continue on to Neptune, providing:

1. No significant sacrifice of Uranus-system science will be incurred in order to so target, and
2. The only permissible change to the spacecraft to increase its longevity will be to assure that consumables are adequate for an extended mission to Neptune.

### 5.4 Finding Number 4

Additional USWG findings in support of a Uranus exploration program are:

1. The 1977 Uranus occultation of the KS star SA0158687 is of great interest and value to Uranus science, and should be actively pursued.



2. The ephemerides of Uranus and its satellites must be updated and maintained in order to achieve the desired targeting precision at Uranus.
3. A "planetary camera" incorporating a 400 x 400 or better CCD array on LST would be extremely valuable for Uranus science.

## APPENDIX B

### REPORT OF THE PIONEER JUPITER ORBITER MISSION DEFINITION GROUP

#### 1. Summary

Principal scientific objectives include comprehensive study of the Jovian magnetosphere by a wide variety of complementary techniques; entry probe measurements of the structure and composition of the atmosphere of the planet to a pressure of at least 10 bars; imaging of the planet and its inner satellites (the latter to a resolution of  $\approx 5$  km); radio occultation measurements of the upper atmosphere and ionosphere of the planet and its satellites; remote sensing by UV and IR techniques; a search for dust belts; and observation of radio emissions. Extensive use is planned of the satellite encounter technique for progressive modification of the orbit of the spacecraft for a variety of purposes. A considerably upgraded spacecraft of Pioneer 10/11 type is envisioned. A formal new start in FY 1977 will make possible launch in December 1980 and arrival at Jupiter in February 1983. Following probe release and entry, a three year orbital mission of some 40 orbits is planned.

#### 2. Introduction

Exploration of the outer solar system became a firm part of U.S. planning in 1967 through the joint efforts of the NASA Office of Planetary Programs and the Lunar and Planetary Missions Board. The tangible results of these early efforts are represented by the Pioneer 10 and Pioneer 11 missions. Pioneer 10 flew by Jupiter in November-December 1973 and Pioneer 11 flew by Jupiter in November-December 1974. The encounter trajectories of the two spacecraft were quite different. The composite results from these two missions have yielded an immense body of detailed observations of diverse types on the planet itself and on its satellites. Substantial publication of new findings has already occurred.

The targeting of Pioneer 11 at Jupiter was such that its subsequent trajectory will lead to a close encounter with Saturn in early September 1979. It is ballistically feasible to then either return to Jupiter (September 1986) or continue outward to Uranus (December 1985).

The only other authorized program of outer solar system exploration comprises the two Mariner Jupiter/Saturn missions, to be launched in August-September 1977 with Jupiter fly-bys April-May 1979 and Saturn fly-bys February-May 1981.

The general rationale for outer solar system missions was summarized in "A Strategy for Investigation of the Outer Solar System--Outer Planets, Their Satellites and Particles and Fields at Great Distances from the Sun" by the Science Advisory Group (1973) as follows:

"(1) Exploration of the outer solar system with automated spacecraft is timely and has high scientific merit. High capability missions in the latter half of the 1970's are technically feasible, using existing launch vehicles. These missions should be aimed toward achieving a fundamental base of knowledge of the outer solar system, so that advanced capabilities in the future can be effectively used for detailed investigation.

"(2) A comprehensive, long range program consists of planetary fly-bys, orbiters and entry probes plus interplanetary and (effectively) interstellar investigations.

"(3) For the scientific and environmental study of any specific planet, a fly-by reconnaissance mission should precede the first orbiter or probe mission to that planet.

"(4) An atmospheric entry probe (i. e., survival to approximately 10 bar) is regarded as the only technique for definitive determination of the elemental and isotopic composition of outer planet atmospheres. Because of competing mission requirements the missions should be 'dedicated' to the probe objectives rather than combined with other planetary mission objectives.

"(5) Jupiter orbiters are of high scientific interest but because of questions of radiation interference and damage, they can not be undertaken with confidence of success until and if Pioneers 10 and/or G show the radiation environment to be much less severe than currently thought possible (or unless periapse is placed beyond  $6 R_J$ ).

"(6) For the same reasons and with the same qualification, it is thought unwise to undertake any multiplanet fly-by missions that require approach to Jupiter closer than  $6 R_J$ .

"Whether it is scientifically preferable to concentrate investigations on a single one of the outer planets (e.g., Jupiter) or to emphasize broad scale exploration of as many planets and satellites as possible (as well as the distant interplanetary medium) has been debated warmly and inconclusively on a number of occasions. On the one hand, it is argued that exploration is the driving function of all scientific work and that the systematics of the physical properties of many planetary bodies of various sizes and distances from the Sun will provide a superior level of general understanding of the origin and evolution of the solar system. Also, wide ranging exploration has a certain popular appeal necessary for the successful pursuit of an expensive, publicly-supported enterprises. On the other hand, many persons consider that the concentrated investigation of a narrower range of specific questions is much more satisfying and scientifically significant. A particular example is the molecular and elemental composition of the deep atmosphere of Jupiter. At the level of recent actual experience, though perhaps less accurately to the point, it is difficult to deny the great superiority of the orbital studies of Mars by Mariner 9 over the fly-by studies by Mariners 4, 6, and 7, though this mission obviously benefited from the previous missions. The only clear conclusion that has emerged is that there is merit on both sides of the issue and that a national strategy should encompass both points-of-view. In the case of Jupiter, this issue was avoidable for initial phases of the exploration program. The radiation environment of Jupiter must be defined before intensive exploration can proceed."

Following the extensive survey of the radiation belts of Jupiter by Pioneer 10, it became realistic to consider Jupiter orbiter missions.

In February 1974, a Pioneer Jupiter Orbiter/Probe mission was identified as a candidate for cooperative conduct by the National Aeronautics and Space Administration and the European Space Research Organization. During a six months' period of 1974, a concentrated study was made of scientific objectives and engineering and technical requirements for a PJO<sub>P</sub> mission. These studies were made by an American/European ad hoc Mission Definition Group; by the NASA/Ames Research Center with TRW Systems Group and with the McDonnell Douglas Astronautics Company - East; by the Jet Propulsion Laboratory; and by ESTEC/ESRO with Messerschmitt-Bölkow-Blohm GMBH. The outcome of these studies was reviewed at the Ames Research Center 11-13 November 1974. In January-February 1975,

the governing board of ESRO decided against European participation in the mission. Subsequent development of plans for the mission has been the responsibility of NASA/Ames Research Center.

### 3. Mission Profile

At the present date, there are uncertainties as to which launch year is financially feasible and on many technical matters of a detailed nature. The tentative plans of NASA are to initiate a Phase B Engineering study in late 1975 and to simultaneously invite specific proposals for scientific experiments. Assuming an FY 1977 start and adequate funding, any one of a variety of options can meet a launch date in early December 1980. Ballistically feasible launch windows exist at about 13-month intervals into 1983.

The following mission profile is illustrative, though not definitive.

- \* Launch: 6 December 1980.
- \* Flight Time to Jupiter: 800 days.
- \* Arrival at Jupiter: 14 February 1983.
- \* Release of Probe and Deflection of Bus: E - 50 days.
- \* Retro-Maneuver at Periapsis: E - 16 min to E + 16 min.
- \* Initial Half-Orbit:  $1.8 \times 150 R_J$ . Near equatorial.  
Apoapsis at about 0700 hours local time.
- \* Moderately close encounters with Amalthea, Io, and Europa are feasible during the first periapsidal pass.
- \* At first apoapsis, a forward impulse raises periapsis to about  $15 R_J$ , or perhaps a lesser value if estimates of radiation damage permit.
- \* Subsequent satellite encounters, principally with Ganymede and Callisto, are used to change the apoapsis and rotate the line of apsides in local time.

- \* Within a three year period the orbital picture can be made to resemble a three-petal flower with one major petal extending to about  $150 R_J$  at local solar times of 0600, 0000, and 1800 hours, respectively. Minor, intermediate petals are of lesser semi-major axis, some being approximately circular. Of the order of 40 orbits and 40 satellite encounters occur during the three-year period.
- \* Radiation damage is estimated from Pioneer 10/11 observations (in to  $1.6 R_J$ ) to be a tolerable though non-trivial problem. Proper phasing of arrival time relative to planetary rotational phase can minimize exposure of both the probe and the orbiter at initial periapsis.
- \* Probe Entry Life: 30 minutes.
- \* Injected Mass (Orbiter and Probe): 1092 kg.
- \* Probe Mass: 150 kg.
- \* Probe Instrument Mass: 20.6 kg.
- \* Orbiter Instrument Mass: 50 kg.
- \* Launch Vehicle: Titan 3E/Centaur D-1T/TE-364-4.

A fuller description of the mission profile is given in the February 1975 NASA/ARC report "Pioneer Jupiter Orbiter with Entry Probe", prepared by TRW under Contract NAS2-8532, and in "Pioneer Jupiter Orbiter Probe Mission - 1980, Probe Description", prepared for NASA by R. E. De Frees of McDonnell Douglas Astronautics Company-East, NASA-CR 137591 of 8 November 1974.

#### 4. Scientific Objectives

##### 4.1 General Objectives

There is already a substantial body of knowledge on Jupiter and its satellites as developed over many years by ground-based techniques, and by the fly-by missions of Pioneer 10 (1973) and Pioneer 11 (1974).

The two MJS missions with fly-bys of Jupiter in 1979 will also precede a PJO<sub>P</sub> mission. The design and choice of scientific instruments are conditioned accordingly.

At the outset of the study, the complement of scientific instruments on a minimal PJO was envisioned as being similar to that on Pioneers 10/11 with the addition of plasma/wave and radio science instruments. However, during the course of the work, a considerable upgrading in the scope and sophistication of the scientific objectives occurred, viz.:

- (1) The prospective survey of the magnetosphere of Jupiter has been enhanced greatly by adoption of the JPL scheme for using satellite fly-bys to "pump" the orbit of the spacecraft and rotate the line of apsides ("flower-power"). This technique makes possible a comprehensive survey of the magnetosphere as a function of radius and local time as well as of real time over an extended period.
2. The second major aspect of the flower-orbit technique is that it provides many close encounters with Ganymede and Callisto and possibly one with JVI. Moderately close encounters with Amalthea, Io, and Europa can occur during the initial pass and others with Io and Europa may be possible later. Thus, there will be a rich variety of opportunities for imaging of the satellites (at  $\approx 5$  km resolution) as well as the planet (at  $\approx 80$  km resolution) at a variety of phase angles and for investigating ionospheres and atmospheres of the planet and its satellites by radio occultation techniques.
3. The inclusion of a deep entry probe (10-20 bars) as a basic element of the approach phase is regarded as an essential feature of the mission in order to provide the first in situ atmospheric data on a Jovian planet.
4. A data storage unit (adopted from the HELIOS program) of  $2 \times 10^6$  bits core storage is a feasible subsystem. Such a unit is a necessity for the upgraded imaging capability that is desired and for probe data storage.

5. Other objectives are similar in nature to those of Pioneer 10/11, augmented by the addition of plasma/wave and radio science instruments. All of the scientific objectives are advanced greatly by the diversity of orbits and viewing opportunities that occur during a prolonged orbital mission of the type envisioned. (A final "cranking" maneuver that tilts the plane of the orbit around its line of apsides to an inclination of about  $30^\circ$  is possible.)
6. There is also a great increase in the potential yield of celestial mechanical information: accurate masses, radio, and shapes of satellites and refinement of their ephemerides.

#### 4.2 Specific Objectives

##### 4.2.1 Orbiter

1. Separation of spatial and temporal variations in the outer magnetosphere.
2. Structure and dynamics of the outer magnetosphere.
3. Structure and temporal stability of the inner radiation zone.
4. Properties of the magnetotail.
5. Topology and dynamics of the magnetopause.
6. Magnetopause-neutral sheet interaction.
7. Nature of energetic particle emission by the magnetosphere.
8. Structure and topology of the bow shock.
9. Distribution of thermal plasma.
10. Magnetospheric effects of satellites - MHD, particle sweeping, injection and acceleration, and the modulation of decametric radio emissions.
11. Ionospheric-magnetospheric coupling.
12. Morphology and dynamics of planetary cloud features.
13. High resolution imaging of satellite surfaces.
14. Global heat balance.
15. Improved satellite ephemerides.



16. Search for and measurement of satellite atmospheres and ionospheres by many S/X band occultations at various phase angles.
17. Auroral emissions.
18. Distribution of dust in the gravitational field.

#### 4.2.2 Entry Probe

1. H/He abundance ratio as a function of depth.
2. Elemental and molecular composition of the deep atmosphere as a function of depth.
3. Pressure-temperature profile of both upper and lower atmospheres.
4. Benchmark data for interpretation of S/X band occultation data.
5. Point-by-point measurements of heat flow.
6. Height and layering of clouds.
7. Ionospheric structure and density.
8. Ionospheric currents and ionospheric-magnetospheric coupling.
9. Energetic particle fluxes down to the top of the atmosphere (inner boundary of the magnetosphere).
10. Higher harmonics of planetary magnetic field.

#### 4.2.3 Pre-Encounter

1. Radial gradient of galactic cosmic ray intensity at a different epoch in the solar activity cycle.
2. Interplanetary magnetic field and solar wind properties including plasma-wave instabilities, also at a different epoch.
3. Propagation of energetic solar particles at large radial distances from the sun.
4. Correlative studies of the above phenomena in conjunction with a very widely spaced network of other outer solar system missions in progress.

## 5. Model Scientific Payload

### 5.1 Orbiter

<u>Instrument</u>	<u>Mass (kg)</u>	<u>Power (Watts)</u>
● Magnetometer	3.0	4.2
● Plasma Probe	9.0	6.5
● Energetic Particles	10.0	6.0
● Plasma/Wave	6.0	6.5
● Radiophysics	1.6	11.0
● Micrometeoroids	2.0	1.8
● Imaging Photopolarimeter Plus UV Photometer and IR Radiometer	9.0	6.0
● Shielding Contingency	9.4	---
Totals	50.0 kg	42.0 watts

## 5.2 Probe

<u>Instrument or Measurement</u>	<u>Mass (kg)</u>	<u>Power (Watts)</u>
● Temperature	0.7	0.2
● Pressure	0.9	0.8
● Accelerometer	0.7	2.0
● Mass Spectrometer	9.0	12.0
● Gas Chromatograph (H <sub>2</sub> /He)	3.0	3.0
● Solar Flux	1.8	4.0
● Infrared Flux	2.3	2.5
● Nephelometer	1.2	2.0
● Energetic Particles	1.0	0.5
Totals	20.6 kg	27.0 watts

APPENDIX C

REPORT OF THE MARINER JUPITER ORBITER  
SCIENCE WORKING GROUP

1. Introduction

The Mariner Jupiter Orbiter Science Working Group was formed late in 1974, and as of this writing (1 June 1975), has met twice to consider the questions associated with the MJO mission. Over this limited period of time, there have been a number of important developments which could influence strategies for intensive exploration of the Jupiter system. The two most important developments seem to be: (a) understanding of the Pioneer 10/11 radio occultation data; (b) detailed evaluation of MJS imaging performance at Jupiter. In both cases, the developments tend to increase the appeal of an early MJO mission, as we shall discuss in detail below.

The SOPE findings are substantially revised from earlier versions, and reflect the increased attractiveness of the MJO mission. Nevertheless, the PJO mission is still given priority as the first Jupiter orbiter. This recommendation can be questioned on the following grounds: (a) the entry probe is not necessarily an integral part of a PJO mission, i.e. one may contemplate probe-only missions; (b) the urgency of entry probe measurements is somewhat reduced by the resolution of the radio-occultation paradox; (c) PJO is not as strongly limited in launch dates as is MJO; (d) the MJO mission has a broader scientific constituency than PJO. For the above reasons, based upon documentation which follows, our group recommends that MJO be the first Jupiter orbiter, to be launched about 2-3/4 years after MJS Jupiter encounter. We adopt this position not out of lack of enthusiasm for the potential of PJO, but because vigorous advocacy of our alternate strategy should improve the chances that a correct choice of mission sequences will be made.

It will be noted that a discussion of imaging plays a prominent role in our report. This is a pivotal part of our argument: the concept that MJS imaging will obviate the need for MJO imaging in the near future seems untenable. There is in fact a very close analogy between MJS-MJO imaging on the one hand and Mariner 6/7 - Mariner 9 imaging on the other. Imaging is a broad-band data gathering system which is ideally suited for exploratory purposes, and we will show that MJO will still be in the exploratory phase. The ease with which such results can be communicated to the public should not be ignored.

We dispute the proposition, that MJO must be substantially upgraded in instrumentation before following on MJS. We maintain that there is a large increment in science return merely from the flexibility and time base available from being in orbit. To proceed directly from a flyby mission and a particles-and-fields emphasis orbiter to, much later, a third-generation planet-emphasis orbiter omits an important intermediate step which should not be neglected for a large and complex system such as Jupiter and its four planet-sized satellites.

## 2. Imaging Capabilities of a Mariner Jupiter Orbiter for Studies of the Jovian Satellites

### 2.1 Goals

The four Galilean satellites of Jupiter are planet-sized objects (from lunar to almost martian dimensions) of great individual diversity about which we at present know very little. What has been learned, particularly from intensive ground-based observations carried out during the past five years, is that each of these is an individual world with a unique past history that has apparently been profoundly influenced by Jupiter, both in terms of its initial composition and its subsequent development. These satellites divide naturally into two groups, somewhat analogous to the terrestrial and jovian planets: Io and Europa are primarily silicate bodies, low in volatiles, and presumably differentiated. Ganymede and Callisto are larger, of lower density, composed primarily of water, and with uncertain internal structure and thermal histories. Each satellite has a unique surface, apparently

differing in composition and structure from the others. There are many puzzles in this picture: for instance, why do Europa and Io, with virtually identical size and density, differ so completely in surface composition? What has been the role of igneous outgassing on each satellite, and is there evidence of water volcanism, or of fluvial erosion? Under what conditions is surface ice stable, and why has the ice not migrated from the equatorial regions to the poles on Ganymede, and why is there little or no surface ice on Callisto? What is the nature of the interaction of each satellite with the jovian magnetospheric particles, and how has this radiation environment influenced the surface properties? Are there tectonic features, or are the surfaces primarily molded by impacts, or are perhaps some of the presumed ice surfaces of insufficient strength to support topographic relief for hundreds of millions of years? It seems clear that here are four new worlds to be explored, worlds quite different from the terrestrial planets yet greater in total area than Mercury, Mars, and the Moon put together.

The Pioneer 10 and 11 flybys of the jovian system have contributed greatly improved masses, a brief but intriguing in situ look at the atmosphere of Io, and an image of Ganymede that is comparable only to the best ground-based views of Mercury. The next opportunity for investigation of the Galilean satellites will be provided by the two MJS missions. The main advantage these will provide over Pioneer 10 and 11 will be in imaging capability; the results for all four satellites will be comparable to the Mariner 6 and 7 coverage of Mars (although somewhat better for Ganymede and Io). As will be discussed in more detail below, it seems unlikely that this information will be sufficient for a detailed study of the geology of these objects, and indeed the Mariner 6 and 7 experience suggests that the MJS data might be quite misleading. The logical post-MJS step in planetological exploration of the jovian system will be a Mariner orbiter, which in terms of imaging will provide a quantum jump for each Galilean satellite over the MJS similar to the quantum jump for Mars provided by Mariner 9 over Mariners 6 and 7. No Pioneer orbiter will provide this gain; in fact, the projected PJO imaging of the satellites would give little

or no improvement over the MJS experiment. If the MJO is not flown in the 1981/82 opportunity, it will be the 1990's before a significant step beyond MJS will be available for the study of the jovian satellites.

To place this discussion in perspective, we summarize below some of the primary scientific objectives of imaging the Galilean satellites and Amalthea. These goals are:

1. Determine the distribution of ices and of silicate or other minerals over the surface.
2. Search for geological evidence of magmatic and/or aqueous volcanism.
3. Characterize any tectonic features and relate them to the internal structure and history.
4. Determine cratering distribution, morphology, and degradation.
5. Determine the thickness of surficial layers, which could be ice over silicate or silicate over ice.
6. Characterize the photometric properties of the surface materials, both ices and silicates.
7. Study the distribution of radiation-influenced albedo and color features.
8. Search for transient or variable surface markings.
9. Search for atmospheric condensations and limb effects.
10. Determine the radius, albedo, large-scale surface markings, and possibly the shape and density of Amalthea.
11. Characterize the morphology of ice deposits, search for evidence of flowage and determine their depositional history.
12. Obtain stereoscopic coverage of at least a portion of each Galilean satellite for accurate height and slope measurements.

We believe that the majority of these goals will not be met by the MJS imaging, and will require the MJO capabilities. We next compare in



some detail the imaging capabilities of the MJS, PJO, and MJO spacecraft for the Galilean satellites.

## 2.2 Comparison of Imaging Capabilities

The ability to interpret correctly the geologic processes and history of a planetary or satellite surface from imaging is directly related to the per cent coverage at a given resolution, and to the illumination angle under which the coverage is obtained. The greater the coverage at high resolution under relatively low sun angle illumination, the more reliable and meaningful is the interpretation. Therefore, the primary objective of any imaging experiment on a solid surface is to obtain the greatest amount of coverage at the highest possible resolution under favorable illumination conditions. This objective is always constrained to one degree or another by (1) the quality of the imaging system, (2) the trajectory or orbit of the spacecraft relative to the planet or satellite, (3) the communication data rate, and (4) conflicts with other experiments.

Experience with the Moon, Mars, and Mercury indicates that imaging over about 80% of the surface with a resolution between 1-3 Km, and about 1-5% coverage between 0.1-0.3 Km resolution -- all within about 45-50° of the terminator -- is required to interpret the history and geologic processes of a surface with a relatively high degree of reliability (approximately current Martian coverage). Therefore, imaging approaching this type of coverage should be the goal for Galilean satellite exploration. A bare minimum for each of the satellites would be approximately that obtained of Mercury from Mariner 10. In this regard it is useful to compare the resolution vs. coverage for the Moon, Mars, and Mercury with that expected for the Galilean satellites from MJS. This comparison is shown in Figure C-1 and Table C-1.

Figure C-1 and Table C-1 show that the coverage at a given resolution for the Galilean satellites from MJS falls far short of that obtained for the Moon from earth-based telescopes. Only at 0.6 Km resolution

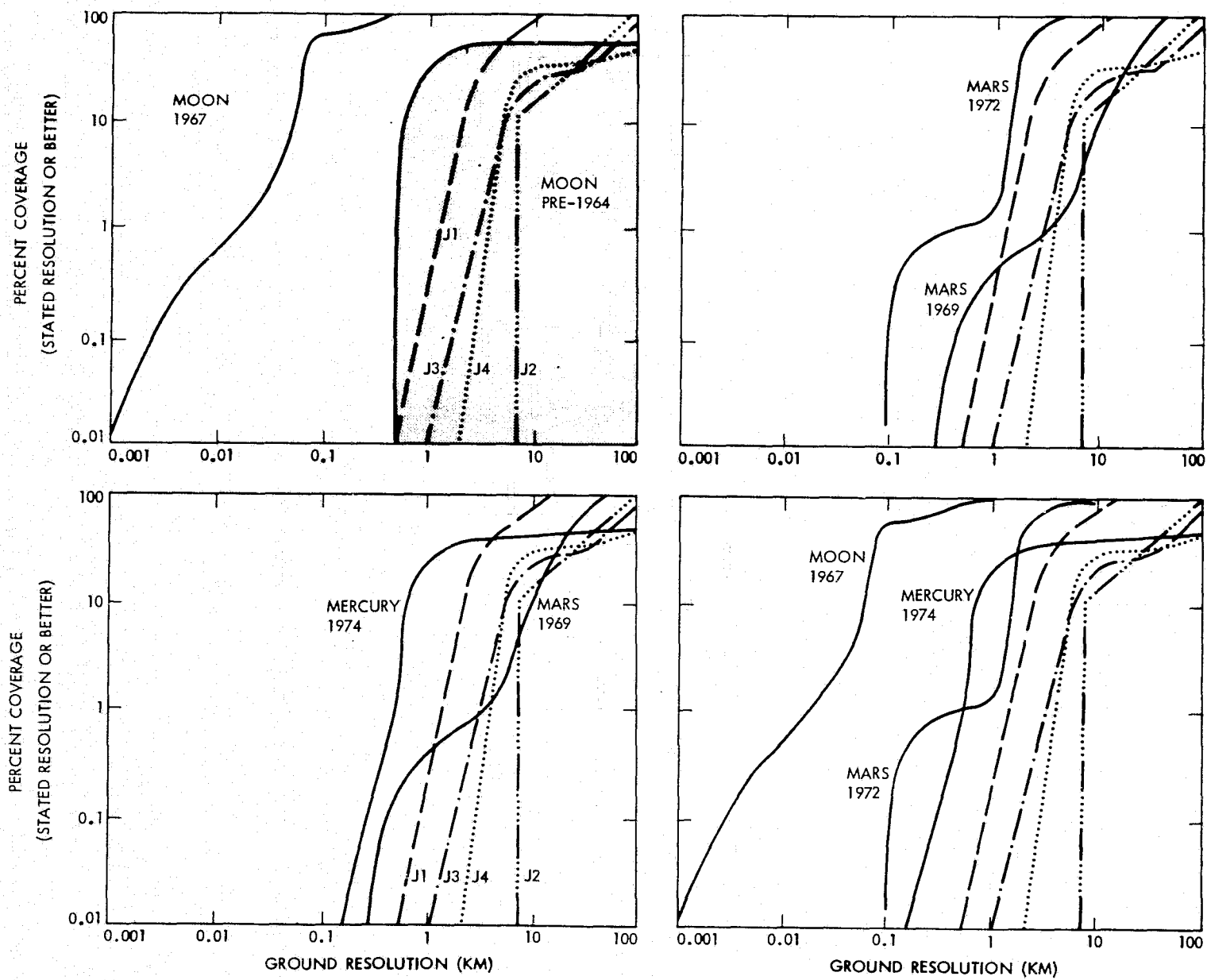


Figure C-1

Table C-1.

Percent Coverage at Stated Resolution or Better	Surface Resolution (Km)								
	Moon	Mars	Moon	Mercury	Mars	MJS <sup>+</sup> (2 S/C)			
	Lunar Orb (4 S/C)	Mariner 9 (1 S/C)	Earth-Based Telescope	Mariner 10* (1 S/C)	Mariner 6 and 7 <sup>□</sup> (2 S/C)	J <sub>1</sub>	J <sub>3</sub>	J <sub>4</sub>	J <sub>2</sub>
100	0.8	2.5	-	-	50	15	120	250	100
50	0.08	1.5	2	4	30	2.5	30	20	40
40	-	-	1	2	20	-	20	15	20
30	-	-	-	1.5	15	-	7	8	10
						4.5			
20	-	-	-	1.0	12	2	4	2	8
10	0.06	-	0.8	0.8	10	1.5	3	-	5
1	0.015	0.2	-	0.6	3.5	1	15	-	-
0.1	0.0013	0.1	-	0.3	0.4	0.8	1.0	-	-
0.01	0.001	-	0.6	0.15	0.3	0.6	0.8	-	-
Earth-based Telescopes									
	2-0.6	100	2-0.6	700	100	~1000			

 Full Phase illumination

\* Third encounter coverage not included (Uncorrected for foreshortening)

+ Data taken from plots supplied by G. E. Danielson (Uncorrected for foreshortening)

□ Uncorrected for foreshortening

do the Moon and Io have comparable coverage at 0.01 per cent.

Furthermore, most of the coverage of the Galilean satellites is at or near full phase illumination, which is virtually useless for discriminating surface structure. Under favorable illumination conditions the Mariner 10 coverage of Mercury at a given resolution is considerably better than is expected on the Galilean satellites. For example, the highest resolution on any of the satellites (Io) is 0.6 Km. At this resolution Mariner 10 coverage of Mercury is 100 times more extensive. The same percentage of Io's surface (0.01) was covered by Mariner 10 at 4 times higher resolution. Under favorable illumination the per cent coverage of Mercury at any given resolution is many times better than expected for the other Galilean satellites. It is highly unlikely that the additional coverage obtained by a single spacecraft MJU mission will raise the coverage at a given resolution to nearly that obtained of Mercury from Mariner 10.

Although the coverage expected from MJS will be a quantum jump in our knowledge of the Galilean satellites, it will fall far short of that required for a detailed analysis such as Mariner 9 of Mars or Mariner 10 of Mercury. The only feasible way of securing extensive high resolution coverage of all the Galilean satellites under favorable illumination conditions is with a satellite-intensive Jupiter orbiter equipped with an imaging system and data rate (~117 Kbs) somewhat comparable to that on Mariner 10. Table C-2 compares the estimated coverage vs. resolution on Ganymede from MJS, MJO and PJO. For comparison the imaging of Mercury from MVM is also shown. The diagonal line pattern indicates coverage near 0° phase angle; all other coverage is near 90° phase angle. Both the MJO and PJO imaging represent 12 encounters with Ganymede, while MJS and MVM each represent two encounters. Clearly MJO imaging is far superior to that of the other missions, and represents a quantum jump over MJS. For instance, MJO coverage of Ganymede is about a factor of 80-500 greater than that of MJS at equivalent resolutions, and the maximum resolution is about 12 times better. As presently structured the PJO mission is primarily oriented toward fields and particles measurements and the insertion of an atmospheric probe with multiple

Table C-2. Comparative Imaging on Ganymede

% Coverage	Resolution (Km)			
	MVM <sup>1</sup>	MJS	MJO <sup>2</sup>	PJO <sup>3</sup>
80.0	-	100.0	2.00	9.0
50.0	4.0	30.0	0.90	5.0
		28.0		
20.0	1.0	4.0	0.75	4.0
10.0	0.8	3.0	0.65	3.0
1.0	0.6	1.5	0.35	1.5
0.1	0.3	1.0	0.08	0.2

<sup>1</sup> Mercury



Full Phase illumination

<sup>2</sup> 800 x 800 CCD, quadrature tour

<sup>3</sup> 160 line CCD, quadrature tour

encounters of Ganymede and Callisto only; there are no close encounters with Io or Europa. For comparative purposes the PJO Ganymede coverage shown in Table C-2 assumes a satellite intensive mission with encounters identical to MJO. The MJO resolution is about 3-5 times better than PJO and comes very close to the maximum imaging goals stated earlier. The coverage vs. resolution on Io would be considerably better since there can be 22 encounters. Notice that the PJO coverage at resolutions of 1.5-4 Km is the same as that of MJS. Clearly the imaging capabilities of MJO are considerably better than those of PJO.

### 2.3 Impact of MJS on MJO Imaging

Within the next 12 years there are two launch windows available for an MJO mission using a Titan III E/Centaur D-IT/propulsion module: December 30, 1981 to Jan 9, 1982, and mid 1987. For the early

launch date, the spacecraft is inserted into orbit in July 1984 and the satellite survey is completed in March 1987. For the late launch date, the first detailed imaging of the Jupiter system does not begin until early 1990 and the satellite survey terminates in 1993. Therefore, if the 1987 launch date is selected there will be about a 10 year hiatus between the reconnaissance imaging obtained by MJS and MJU, and the detailed imaging acquired by a MJO. From an imaging standpoint the early launch date is much preferred.

One reason suggested for deferring MJO to a 1987 launch is the need to assess the MJS results before planning an optimum MJO experiment. This argument undoubtedly has merit with respect to the possible development of entirely new experiments for an MJO mission, but it does not apply to imaging. The key to study of the satellite surfaces by imaging is resolution and coverage, and there is no question of the need for improvement in both of these over the MJS performance. Detailed mapping of the surface of each Galilean satellite will be required independent of the MJS results.

It is certainly possible, and perhaps probable, that MJS imaging will reveal fundamental differences among the Galilean satellites that would impact the details of the exploration strategy. It may be, for instance, that at MJS resolution one or more Galilean satellite may turn out to be devoid of topographic features. Such a situation could occur on a completely ice-covered satellite and would be in some ways analogous to observing Earth from the Pacific Ocean side or imaging Mars at the height of a planet-wide dust storm. But even in this worst case, we are convinced that one would not fail to use the higher resolution and greater coverage of the MJO to explore an object that had appeared featureless at MJS resolution. Given the great diversity among the Galilean satellites and the present ground-based and Pioneer 10 evidence for surface markings on all of them, we consider a pathological situation in which none of the satellites was of interest from the point of view of imaging to be practically inconceivable. And even in this case, the MJO would still carry the best available imaging system for studies of Jupiter.

The above arguments suggest to us that no MJS results will have major impact on MJO imaging hardware. MJS results may, however, influence mission strategy. But this is easy to accommodate, and in fact the MJO mission is designed for continuous interaction with its own results in planning the sequence and nature of satellite flybys. Thus there will be no difficulty whatever of incorporating MJS results (to be obtained nearly 5 years before a 1981/82 MJO orbits Jupiter) in MJO mission planning. Furthermore, there will be almost 2-3/4 years between the MJS Jupiter encounter and the MJO launch, which is ample time to modify the mission profile, change filters, or retarget the cameras.

#### 2.4 Conclusions

1. With presently available hardware or CCD systems which will shortly be available, the imaging obtainable from MJO of the Galilean satellites and probably Amalthea can result in a very significant gain in our knowledge of these bodies over that attainable from MJS, MJU or PJOp, or a combination of these missions.
2. There is sufficient time between the MJS Jupiter encounter and a 1981/82 MJO launch to modify the MJO mission profile, filter selection and imaging sequence if the MJS results warrant such changes. The selection of an MJO imaging system is not dependent on the results of the MJS mission.
3. In view of conclusions 1 and 2 and the long time interval between reconnaissance and detailed imaging which would result from a late (1987) MJO launch, it is recommended that the MJO mission be flown during the 1981-82 launch window. Even with this early launch date the nominal mission (much less an extended mission) would not terminate until about mid-1987 and a relatively detailed photographic analysis of the Jupiter system would not be completed until about late 1988 or 1989.

### 3. Observations of Jupiter's Atmosphere from an MJO

#### 3.1 Introduction

This section is an outgrowth of the MJOSWG meeting on May 7-8, 1975. An attempt is made to summarize the important science that will not have been done after MJS, but which could be done from an orbiter using MJS or other spacecraft-tested equipment.

#### 3.2 Studies of Atmospheric Dynamics

The dynamics are important not only for their intrinsic scientific interest -- the organization, long lifetime and regularity of Jovian atmospheric features are phenomena that do not occur on earth, and hence could aid in our basic understanding of atmospheric dynamics. In addition, the dynamics may affect the results of many in situ or remote-sensing measurements of composition, temperatures, cloud structures, etc. To understand the dynamics one needs four-dimensional coverage of the planet-coverage in space and time. Only an orbiter with high-resolution imaging can provide this coverage.

MJS will provide about 100 hrs. of coverage at better than 100 km resolution on Jupiter. Given the long time scales of phenomena on Jupiter, this still may be just a snapshot. To understand the dynamics one needs to study several regions for a year or more at 100 km resolutions or better, to watch structures evolve and decay, to measure transports of momentum, and to observe horizontal convergence and divergence. Repeated snapshots on successive orbits at 10 km resolution would also help to understand structures of size equal to the atmospheric scale height.

#### 3.3 Studies of Temperatures and Energy Flux

The same comments concerning four-dimensional coverage apply to temperatures and energy flux. In this respect, an orbiter is not a repeat of an MJS mission. The same instruments on an orbiter will provide answers to many questions that MJS cannot provide. An



orbiter capable of concurrent thermal mapping coupled with high resolution imaging will be exploring phenomena in the time domain that cannot be anticipated even after MJS and MJU results are analyzed.

For a Jupiter orbiter, one would prefer a thermal sounding experiment at a variety of infrared and microwave frequencies to probe the thermal and cloud structure down to the 10 bar level. In this way one can expect to measure horizontal temperature differences -- which can be related to winds observed by the imaging -- as well as the radiative fluxes which ultimately cause the temperature differences. An orbiter will measure the range of variation of temperature and pressure, which otherwise might complicate interpretation of a single in situ measurement.

#### 3.4 Studies of Chemical Composition

Clearly an entry probe is best suited for measuring isotope ratios and trace constituents, provided reliable instruments can be developed. The variable constituents, including  $H_2O$ ,  $NH_3$ ,  $H_2S$ , cloud particles, chromophores, etc. must be mapped in space and time if one is to understand their abundance in the atmosphere.

It is becoming increasingly evident, but hardly surprising, that the chemical equilibrium in Jupiter is dynamic rather than static. Bar-Nun (Icarus 24, 86-94 (1975)) has demonstrated that neither acetylene nor ammonia could exist in a static Jupiter. He suggests that thunderstorm shock waves can be extremely productive sources of nonequilibrium species, as well as the usual deep-mixing generation. His mechanism requires the thunderstorm activity to be  $\sim 10^4$  as dense as on Earth. If this be the case then there is almost no limit to the conceivable molecules that can be generated. It is important, therefore, to make a thorough study of the abundance, distribution and motions of as many of these trace constituents as possible. However, the abundances of these constituents are certainly low,  $10^{-9}$  to  $10^{-12}$  or less. In order, then, to detect them, long paths are necessary.

Jupiter offers two ways of achieving this: the  $5\mu$  hot-spots and solar occultations. Both have their attractions and difficulties.

The  $5\mu$  hot-spots are believed to be the consequence of fortuitous coincidences of gaps in the various Jovian cloud layers and a low atmospheric extinction. The holes can persist for days, or even weeks, but their size is unknown. They are certainly less than 3000 km in extent and could be as small as a few hundred km. (Westphal, private communication). However, a remote sensor capable of viewing a single "hole" for significant periods (up to 20 minutes, say) would have an unparalleled view into the interior of Jupiter, perhaps even to the 20 bar level. Such an observation is impossible from Earth or near-Earth because of the small size of the features. Furthermore, according to Westphal (private communication), the spectral region is not totally free from reflected sunlight. The ability to make the observation in the night-time hemisphere may therefore be important.

Calculations show that an infrared spectrometer could obtain  $0.1 \text{ cm}^{-1}$  resolution spectra in the  $5\mu$  ( $2000 \text{ cm}^{-1}$ ) region with signal-to-noise ratios of several hundred in 20 minutes. A resolution of  $0.1 \text{ cm}^{-1}$  should suffice because, at 20 bars pressure, linewidths would typically be  $\sim 1 \text{ cm}^{-1}$ . That is, the lines should be well resolved. At that level, all the information derivable from spectroscopy becomes available. The same experiment would also substantiate the nature of the hot-spots. If the hot-spots truly do permit penetration to great depth and temperature, the  $5\mu$  window in the hot-spot should be narrower than the window seen when looking at a nearby area because the window cut-off is caused by the P-branch of the  $\nu_3 \text{CH}_4$  band (at the  $4\mu$  end).

The principal difficulty in performing spectroscopy in a Jovian hot-spot will be one of acquisition and guiding. Not only must a hot-spot be found, but the spectrometer must be pointed at it and held, with milliradian accuracy, for up to 20 minutes. This requirement will place severe demands not only on the scan platform but also on the orbital characteristics of the spacecraft itself. It also presupposes

a high spatial resolution  $5\mu$  radiometer, first to find the hot-spot and then to provide the pointing information to the scan platform and to the fine-guiding device (presumably a wobble mirror).

The other means of attaining long paths is the solar occultation method. The principal advantage is a very long path at low pressure. The absorption lines are therefore narrower, permitting weaker lines to be detected. The other advantage is, of course, that the Sun is being used as a source. Signal-to-noise ratio is unlikely to be a problem. The disadvantages are:

1. The time available for any given measurement is likely to be short, the exact time depending on the orbital characteristics of the spacecraft and the permissible variation in path length.
2. The path is neither straight (because of refraction) nor homogeneous (because of the curvature of the planet).
3. The "sample" is some kind of average across a large segment of the planet.
4. The method is useable only in the upper troposphere. Clouds are likely to cause serious problems from obscuration, forward scattering and specular reflection.
5. Only two occultations, at most, per orbit are possible. If the orbital period is  $\sim 100$  days, little data will be returned.

However, there is no reason why one spectrometer should not perform both experiments and it would seem reasonable, engineering problems aside, to attempt both because they provide complementary data.

All the foregoing has been strongly biased toward  $5\mu$  spectrometry because of the manifest virtues of employing the  $5\mu$  window. However, the very fact that there is a  $5\mu$  window implies that the absorptions in that region are not very strong. This is not a coincidence. An examination of the familiar bar-charts of strong infrared absorption bands shows that, for a remarkable range of molecular species, the region  $1900-2200\text{ cm}^{-1}$  is a zone of avoidance (but, fortunately, not

for the species we seek). It follows, parenthetically, that we can expect a  $5\mu$  window in any planet that is cloud-free or at least has broken cloud. The Earth, Mars and Jupiter have  $5\mu$  windows. Saturn and Venus do not because their cloud decks are unbroken.

In view of the infrequent absorptions in the  $5\mu$  region, any spectrometer flown should also be capable of operations at longer wavelengths, perhaps to  $16\mu$ . The available path-lengths, except for solar occultations, are shorter but the bands are stronger. For certain species, such as  $C_2H_6$ ,  $C_2H_2$  and  $PH_3$  the advantage may be crucial. The short-wavelength limit should be such as to make certain that the entire  $5\mu$  window is covered.  $3.3\mu$  ( $3000\text{ cm}^{-1}$ ) would seem to be reasonable. It is hardly worthwhile going to shorter wavelengths with this kind of instrument because reflected sunlight will confuse the analysis. A possible exception to this statement is the  $2.7\mu$  region, where another small window should exist. The region should be totally solar-dominated so that there should be a strong contrast between holes and clouds. On the other hand, it would be foolish to compromise the long-wavelength experiment by adding this as a vital requirements. The matter requires further study.

The basic infrared spectrometer has now been specified:

Wavelength Coverage: 3.3 to 16 microns ( $600 - 3000\text{ cm}^{-1}$ )

Resolution:  $0.1\text{ cm}^{-1}$

Number of Spectral Elements: 24,000

Maximum Integration Time: 1000 seconds

Minimum Integration Time: 100 seconds

It is not sensible, at the present juncture, to define an instrument. The chief competitors would seem to be either a multichannel grating spectrometer (not necessarily employing all the 24,000 channels available) or a Fourier spectrometer. The grating spectrometer is mechanically simpler and could be conceived without moving parts. The data rates would also be low (a few hundred bits per second). A Fourier spectrometer can cover the entire spectral region (in

principle) without gaps but is mechanically complex and data rates of several thousand bits per second would be generated. Nor are these the only possibilities: systems such as the Hadamard Transform Spectrometer and heterodyne spectrometers would also need to be investigated in the light of a rapidly-changing technology.

The experiments outlined are follow-on investigations to the earlier fly-by missions. These missions cannot, of course, provide data at anything like the resolution, signal-to-noise and volume required here. In a somewhat different sense, the experiments follow the path laid down by ground-based and aircraft observations, from which most of our present spectroscopic data has been produced. The sequence follows directly along the lines of an orderly program of exploratory and planetary-intensive studies. However, neither of the experiments suggested here can ever be performed from Earth: the solar occultation for obvious reasons and the  $5\mu$  hot-spot investigation because the hot-spots are below Earth-based resolution and may be below telescope-in-Earth-orbit resolution, even assuming that such will ever come to pass for infrared use. The experiments are therefore specifically orbiter-oriented and produce some data available in no other way and some that foreshadow and complement later entry studies.

In principle, the trace-abundance and isotopic studies are best performed by entry probes employing mass spectrometers or other automatic chemical laboratories. It does, however, remain to be demonstrated that the substantial technical difficulties of inserting suitable probes can be overcome and that, even if they can, an instrument such as a mass spectrometer can make more accurate measurements of species at the  $10^{-10}$  to  $10^{-12}$  abundance level than can remote sensors. Furthermore, unless the probe is almost stationary in the atmosphere (i.e. moving at less than a few hundred meters per second), the probe itself may destroy the very phenomena sought: trace, weakly-bound, molecules. The very fact that a remote sensor is remote may be a virtue rather than a handicap.

### 3.5 Comparative Imaging on MJO and PJO

The enclosed Figures (C-2 and C-3) and Table (C-3) summarize the capabilities of MJO, MJS, PJO and Pioneer 10/11 imaging systems. In the figures, resolution on Jupiter is given in km per line pair for narrow-angle (NALP), wide-angle (WALP) and line-scan imaging (LSILP), as a function of time from closest approach. The MJO and MJS systems have comparable resolution, but MJO employs CCD cameras, which have greater spectral range (3500-8000Å), shorter cycle time, and greater sensitivity than the MJS videcon system.

The table compares the time and areal coverage at 12 and 100 km resolution for MJO and PJO. The nominal PJO mission is the "flower tour." The time coverage per orbit for MJO is >100 hrs, which is almost continuous coverage. In contrast, the resolution at closest approach for the nominal PJO is 210 km, compared with 7 km for MJO.

The total number of images contemplated for an MJO is large but manageable. One frame per hour would provide continuous coverage at 100 km resolution of a band approximately 90° wide in latitude and 360° in longitude. For a 3.5 year mission, this amounts to 36,000 frames. During closest approach, one could mosaic a region 45° in latitude by 45° in longitude at 10 km resolution with 150 frames. This could be done for 60 closest approaches, for a total of 9,000 frames. Thus the total number of frames of Jupiter in a maximum mission would be 45,000, about 5 times the number of frames of Venus and Mercury in the Mariner 10 mission.

The valuable science that could be done from imaging in a more modest mission is still far above that of MJS or PJO, because of the much longer time span.

### 3.6 Summary

Only an orbiter capable of high-resolution imaging and thermal mapping can resolve many scientific questions about Jupiter. An

TIME FROM CLOSEST APPROACH, hrs

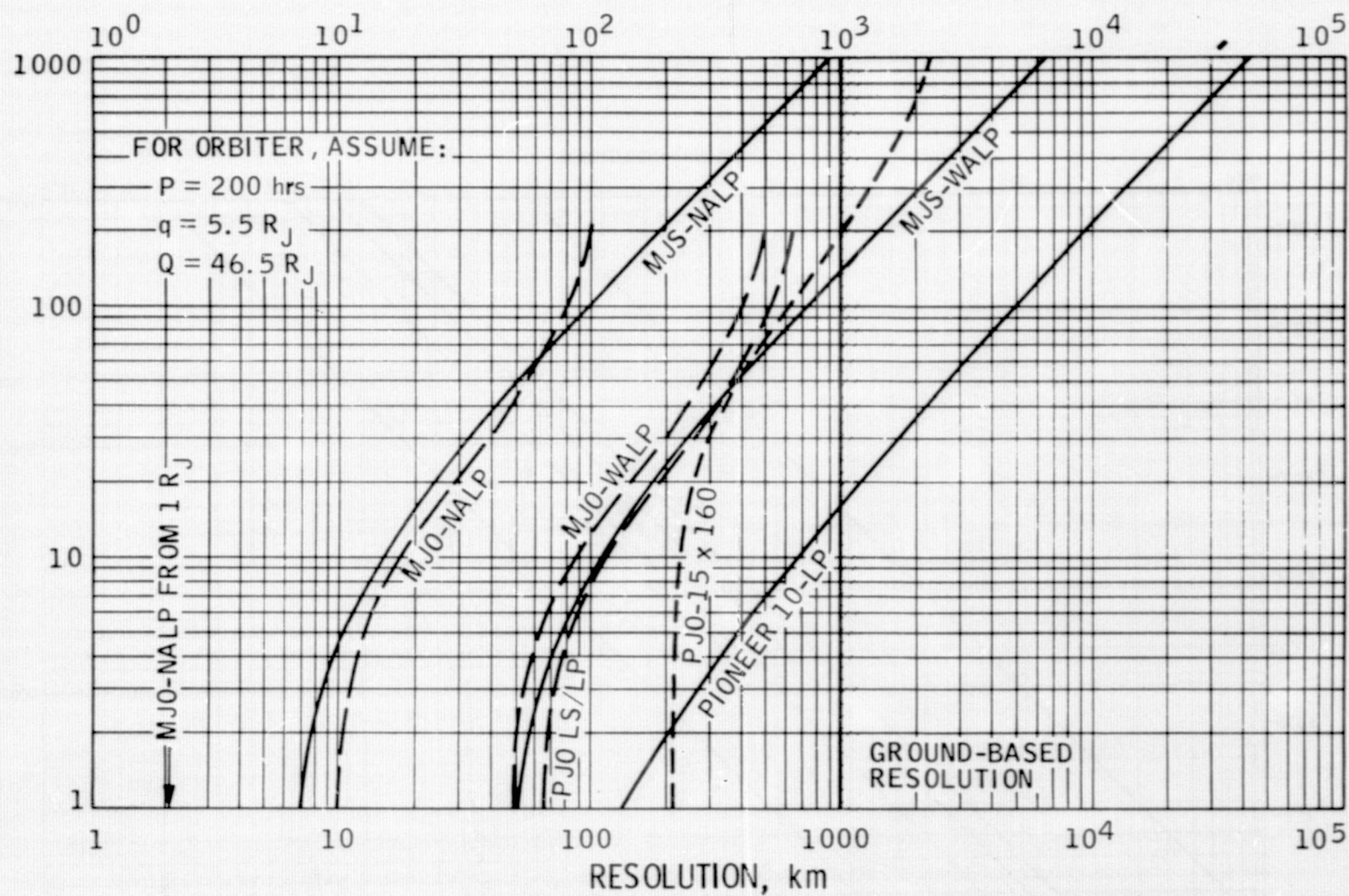


Figure C-2



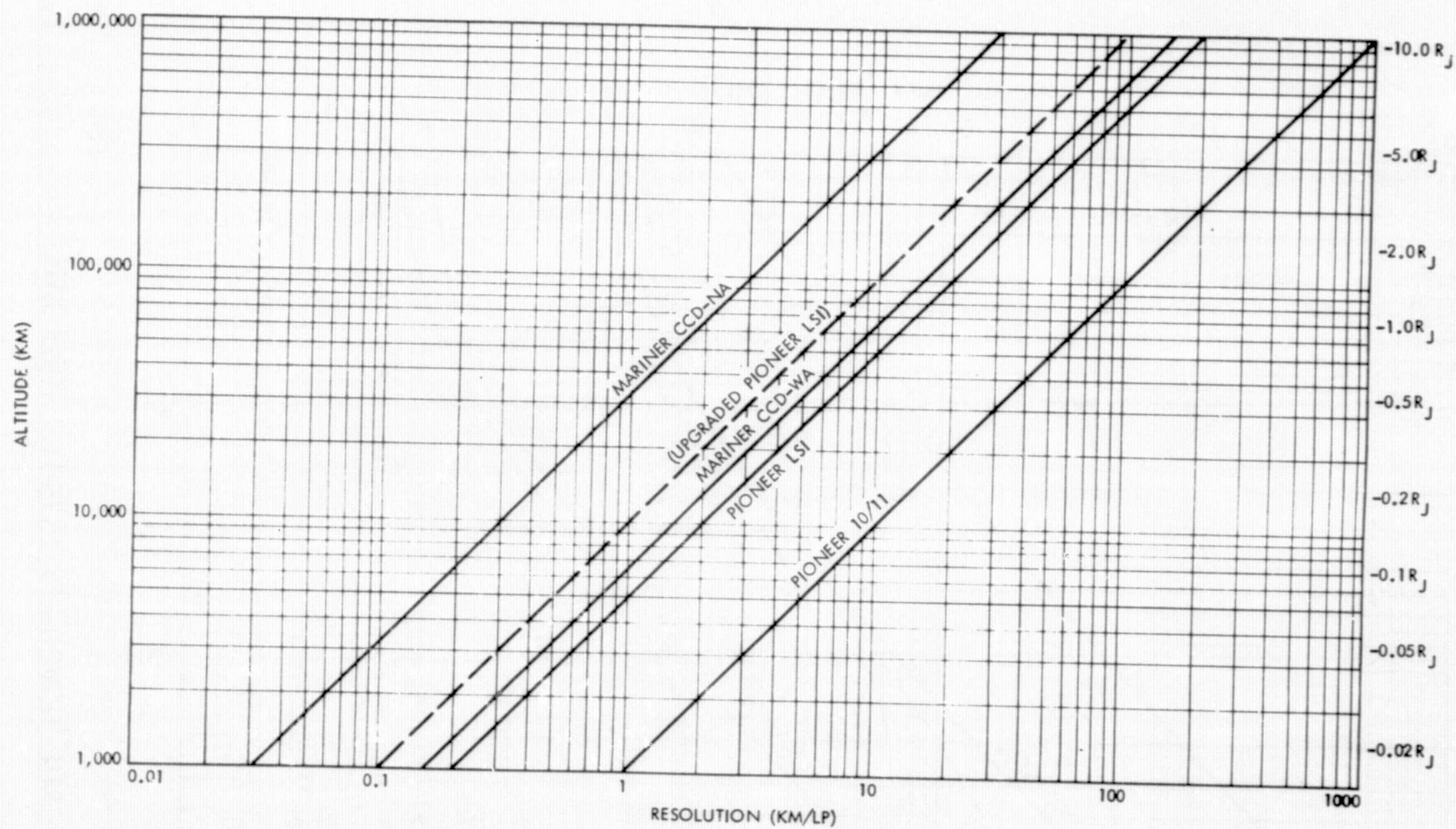


Figure C-3



Table C-3. Comparative Jupiter Imaging

	MJO (Quad. Tour)		PJOp (Quad. Tour)	PJOp (Flower Tour)	PJOp (Upgraded Imaging system) Quad. Tour	PJOp (Upgraded Imaging system) Flower Tour	LST
	Narrow Angle (150cm fL)	Wide Angle (30cm fL)	(23cm fL)	(23cm fL)			
Time coverage/ orbit at <u>12km</u> resolution or better	4-8 hrs	0	0	0	0	0	0
Time coverage/ orbit at <u>100km</u> resolution or better	>100 hrs	12-24 hrs	8-16 hrs	0	20-40 hrs	0	0
Resolution at closest approach (km/line pair)	~7 km	~50 km	~70 km	~210 km	~35 km	~105 km	~400
Areal coverage/ orbit to $\pm 60^\circ$ lat. at <u>12km</u> resolu- tion or better	25%	0	0	0	0	0	0
Areal coverage/ orbit to $\pm 60^\circ$ lat. at <u>100km</u> resolu- tion or better	100%	100%	~80-100%	0	100%	0	0
Total orbits	60	60	60	40	60	40	-
Time interval between encoun- ters (days)	11-21	11-21	11-21	14-100	11-21	14-100	?

orbiter with MJS instruments would provide data of a new and important nature. Thus such a spacecraft should be considered as a valuable and important mission for the near term (1981-82).

#### 4. Fields and Particles Investigations

The investigations of magnetic fields, magnetospheric plasmas, trapped radiation, plasma waves and radio emissions share a set of interrelated scientific objectives. Further study of the magnetic field of Jupiter and its secular changes should provide significant new insights into the origin of planetary dynamos as well as the internal structure of the planet. The possible intrinsic magnetization of the Galilean satellites would have similar consequences. If the satellites are unmagnetized, their interaction with the Jovian magnetosphere can be expected to contribute new information about the electrical conductivity of the satellite surfaces or interiors. The nature of the satellite-magnetosphere interaction should also identify the dominant modes which govern the behavior of the fields and particles and permit inferences to be drawn regarding the precipitation and diffusion of trapped particles as well as the production of the radio noise bursts. Repetitive measurements of the Jovian ring current and other distinctive features of the magnetosphere will allow temporal and spatial effects to be distinguished and will permit a significant advance in understanding the structure and dynamics of the Jovian plasma environment which is very different from that of Earth. Significant progress can be expected in understanding important details of the origin of the synchrotron radiation and decametric emissions with subsequent possible application to other situations and systems throughout the universe. Adequate instrumentation exists in all these areas, such investigations having either been carried out near Earth or on Pioneer, or are now scheduled for MJS. Presently planned, or contemplated, flybys of Jupiter, such as MJS or out-of-the-ecliptic missions, are not a substitute for an orbiter mission which has the potential to go significantly nearer Jupiter and which could encounter all the major satellites, pass through previously inaccessible regions of the magnetosphere and, by virtue of repeated traversals, separate spatial from temporal effects.

## 5. Radio Occultation Science

### 5.1 Comparison of MJO and PJO for Radio Occultation

The accuracy and resolution of radio occultation measurements, and hence of derived atmospheric structure, are greater for MJO than for PJO. Three areas of comparison are shown in the chart. There are strong pressures based on minimizing cost to use Pioneer 10/11 design as much as possible on PJO, so changing from the middle to the right column would be difficult. The middle column makes radio occultation of marginal importance. Improved Pioneer would be good. MJO would be decidedly better, based on higher signal strength and probable superiority in limb tracking capability.

	MJO	PJO (P 10/11 design)	Possible Improved PJO
Oscillator stability	$5 \times 10^{-12}$	$2 \times 10^{-10}$	$5 \times 10^{-12}$
Signal-to-noise ratio	47db (S) 57db (X)	32db no X	40db (5) 50db? (X)
Limb tracking/ <u>ease</u> accuracy	relatively straight- <u>forward</u> needs improvement	relatively difficult, may not <u>be</u> feasible ?	relatively <u>difficult</u> ?

### 5.2 Utility of Simultaneous Remote Sensing

Either Jupiter orbiter mission will produce a large number of radio occultations distributed over the limb of Jupiter, at relatively large limb-spacecraft separations. It follows that interpretation of the data will be sensitive to the geometric factors which operated upon the Pioneer 10/11 occultations. While most of these factors can probably be successfully evaluated, the occultations will be most definitive if one has available simultaneous (or nearly simultaneous) information about the rotation speed and infrared emission from each occultation

point. In other words, intercomparison of occultation temperature profiles with remote temperature sounding and atmospheric images (e.g. stereo information on cloud altitudes) will greatly enhance the value of each experiment. It is clear that this goal will be most readily achieved using the Mariner orbiter.

### 5.3 Additional Radio Experiments

The MJO complement of radio equipment, the mission plan involving numerous close encounters with the major satellites, and the spacecraft capability for the required orientation maneuvers makes MJO very well suited for bistatic radar studies of the Galilean satellites. This experiment provides information on surface slopes and vertical structure of surface density, at a set of scales unavailable by other means.

The magnetic field of Jupiter close to the surface of the planet can be measured by the addition of a Faraday rotation experiment. With two frequencies, one obtains a column electron density and the column average of the electron density times the longitudinal magnetic field.

## APPENDIX D

### REPORT OF THE OUTER PLANETS ENTRY PROBE SCIENCE STUDY GROUP

#### 1. Introduction

Primary objectives for exploration in the outer solar system are the atmospheres of the planets and satellites. The Jovian planets are gaseous to very great depths, so that measurements are largely confined to their atmospheres. By the same token, however, these massive atmospheres are major geochemical features, much more representative of the body as a whole than the wisp of gas we are used to at home. We can therefore expect the atmospheres to contain a great deal of fundamental information about the planets and the solar system.

What methods should be stressed in a program of planetary measurements? Until recently, for planets other than the Earth, there has been a nearly total reliance on remote sensing, and some remarkable successes have been achieved, especially for Mars. But this object has a solid surface, with a wealth of permanent detail, and a thin atmosphere. A very deep atmosphere is a different matter, and even on Venus there is already a heavy emphasis on direct measurements from probes and a low-periapse orbiter. The rest of this document will examine the limitations of remote sensing at and beyond Jupiter, as illustrated by the experience with Pioneers 10 and 11. The demonstrated and expected capabilities of direct measurement will then be discussed, along with an assessment of the state of the art in both instruments and vehicles.

#### 2. Remote Sensing

We shall limit ourselves to measurements that bear on the specific, basic objectives of composition and structure. The classic methods have been visible and near-infrared spectroscopy and thermal-infrared measurements. The former, applied from the Earth, seems to be already getting close to its limit. A current argument over the methane abundance on Uranus gives an interesting example. Methane

bands are exceedingly strong there, but two explanations are possible: the methane abundance may be large, or the atmosphere may be unusually clear. Such ambiguities run through the whole of remote sensing; at any time there may be an accepted result, but it can be greatly changed by a different interpretation. In terms of our confidence in an answer, the error distribution has broad wings, even though it may have a narrow core. This picture can be greatly changed by a single direct, unambiguous measurement; such "ground truth" can also make the indirect data enormously more valuable.

Before the encounter of Pioneer 10, the radio-occultation method was the outstanding example of a "clean" remote-sensing experiment. Even on Mars, however, recent suggestions have reminded us of a fundamental limitation: the temperatures derived depend on the assumed composition, and conversely, any arguments about the composition are highly indirect. A considerable quantity of argon could still be present on Mars without being detected, because its atomic mass is so close to the molecular mass of  $\text{CO}_2$ .

At Jupiter, the method initially failed completely to give valid data on the neutral atmosphere. Attempts were made to reconcile the very high temperatures obtained by Pioneer 10 and 11 with other lines of evidence, but before long a really telling argument, due to Gulkis, became apparent. At 13 cm, the wavelength of the occultation experiment, the brightness temperature of Jupiter observed from the Earth is 280 K. Thus, there is a level in the atmosphere at this temperature that is essentially opaque (optical depth about unity) at 13 cm. If it is opaque for thermal radiation to the zenith, it is certainly highly opaque to any probing wave in a horizontal direction. Any occultation experiment at 13 cm cannot obtain valid temperatures greater than 280 K. (This argument does not apply to an optically thin region such as the ionosphere.)

Very recently, Hubbard, following up some work by Hunten, had found the explanation of the error. The data reduction is extremely

sensitive to the exact angle between the spacecraft path and the limb. Neglect of Jupiter's oblateness had led to a serious error in this angle. The important factor is that the spacecraft tends to be very far behind the limb in the outer solar system: for the Pioneer 10 entry this distance was 220,000 km; for Io it was 550,000 km; for Mars and Venus it was often well under 5000 km. This situation puts extraordinary demands on the data processing, and on the knowledge of the direction of the vertical at the occultation point, small errors being multiplied by factors of 10 or greater due to cancellation. The major error has been found, and reasonable temperatures are being derived. It remains to be seen how good they will be, in the face of the error amplification that is still present. In particular, the experiment is sensitive to distortion of the ray path by random density gradients in the atmosphere.

One partial cure for future missions would be to work at shorter distances. But the nature of Jovian planets, and the trajectories typical of multiple-planet missions, suggest that large distances will continue to be used. At Jupiter, this tendency is reinforced by the radiation hazard. Other objects may or may not have smoother atmospheres than Jupiter; we do not know. The occultation experiment is still valuable, but it has ceased to occupy its unique position of reliability. At best it seems that the results will be strongly model-dependent, as with other experiments. We should like to acknowledge the cooperation of A. J. Kliore in furnishing unpublished data for evaluation of the occultation experiment.

The infrared emission of a planet is full of information; but again two factors are involved. They are the emissivity or opacity of the atmosphere, and the Planck function, which depends on temperature. The auxiliary information needed to disentangle these two can be partially obtained from the limb darkening. The quick-look interpretation of the Pioneer 10 data, based on pre-calculated models, showed internal inconsistencies. More detailed work, recently completed by Orton, gives the temperature profile shown by the solid lines in Figure D-1.

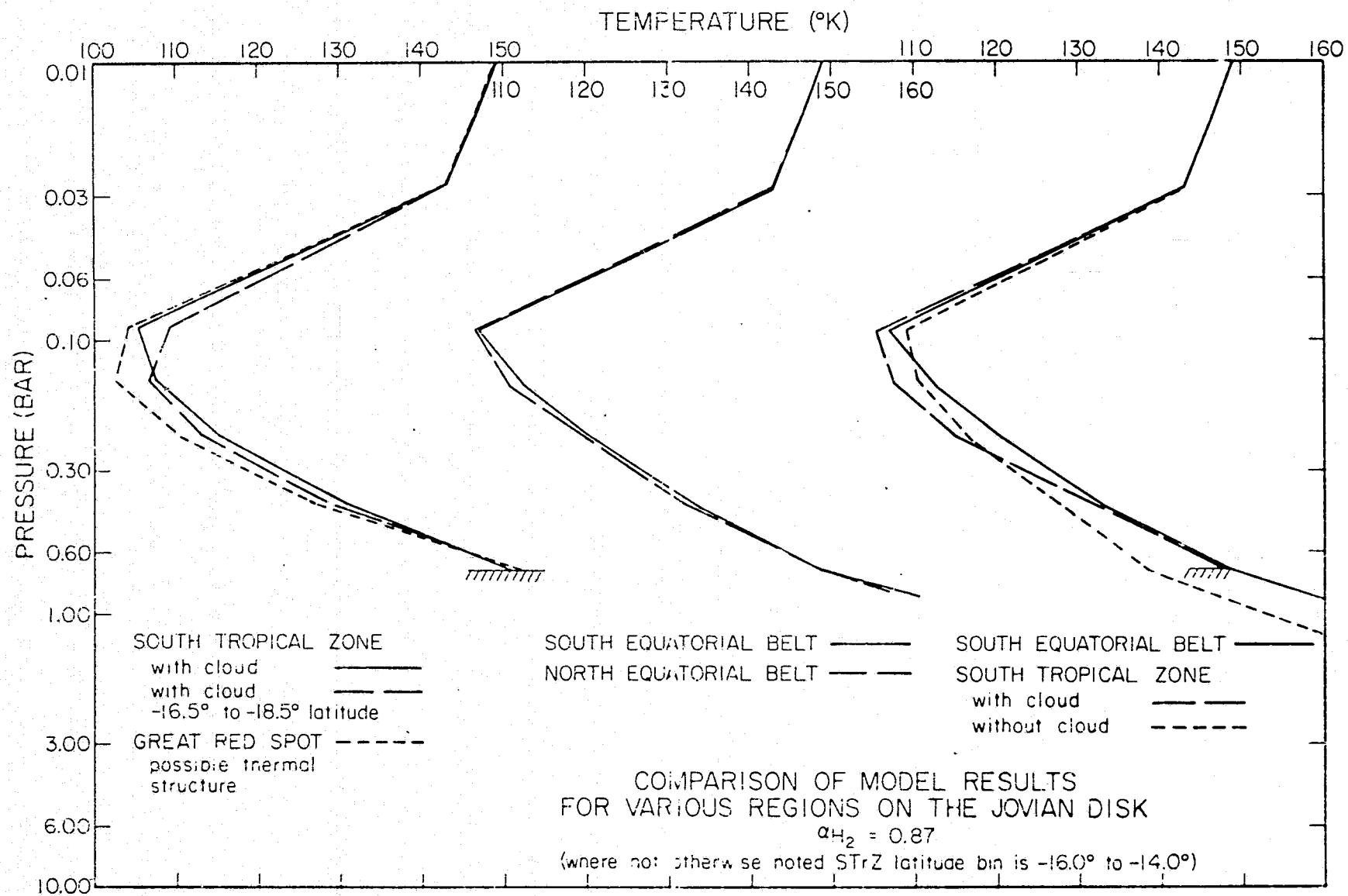


Figure D-1



These results fit very well to the other data shown by dashed and dotted lines. The other stated objective of the experiment was to put limits on the helium abundance. As Figure D-2 shows, this limit is little better than 50%. A more capable infrared experiment could certainly do better, but at some point will run into the more fundamental problem of contributions to the opacity from minor constituents or cloud particles. Our ignorance of these details must always limit our confidence in the derived temperatures and helium abundance.

For a planet as cold as Uranus, an additional difficulty appears: at 60 K, the radiated power is very small, and it is confined to longer wavelengths. An infrared experiment is harder to do, and inherently less productive, than at Jupiter. With the additional, assured information from an entry probe, the value of the experiment is vastly increased.

### 3. Direct Sensing

Implementation of probe technology is well under way in the Pioneer Venus mission. Its extension to the outer solar system has had considerable study in industry and at the Ames Research Center.

OPEPSSG is satisfied that there are no unsolved technical problems in safely entering any of the atmospheres, including that of Jupiter. A typical mission would carry 20-30 kg of scientific instruments, taking an hour or less to traverse the region from 0.1 to 10 bars. A very capable payload can be assembled within this allotment, though many other valuable experiments would have to be left out.

The full report of OPEPSSG contains many more details about a typical probe, its mission sequence, and candidate experiments. Nearly all the instruments we suggest will soon exist as flight hardware through Pioneer Venus; other possibilities come from earlier programs such as PAET (Planetary Atmosphere Experiment Test) and laboratory development.

The obvious advantages of probe experiments are direct, unambiguous measurements, versatility, and operation over a wide range of

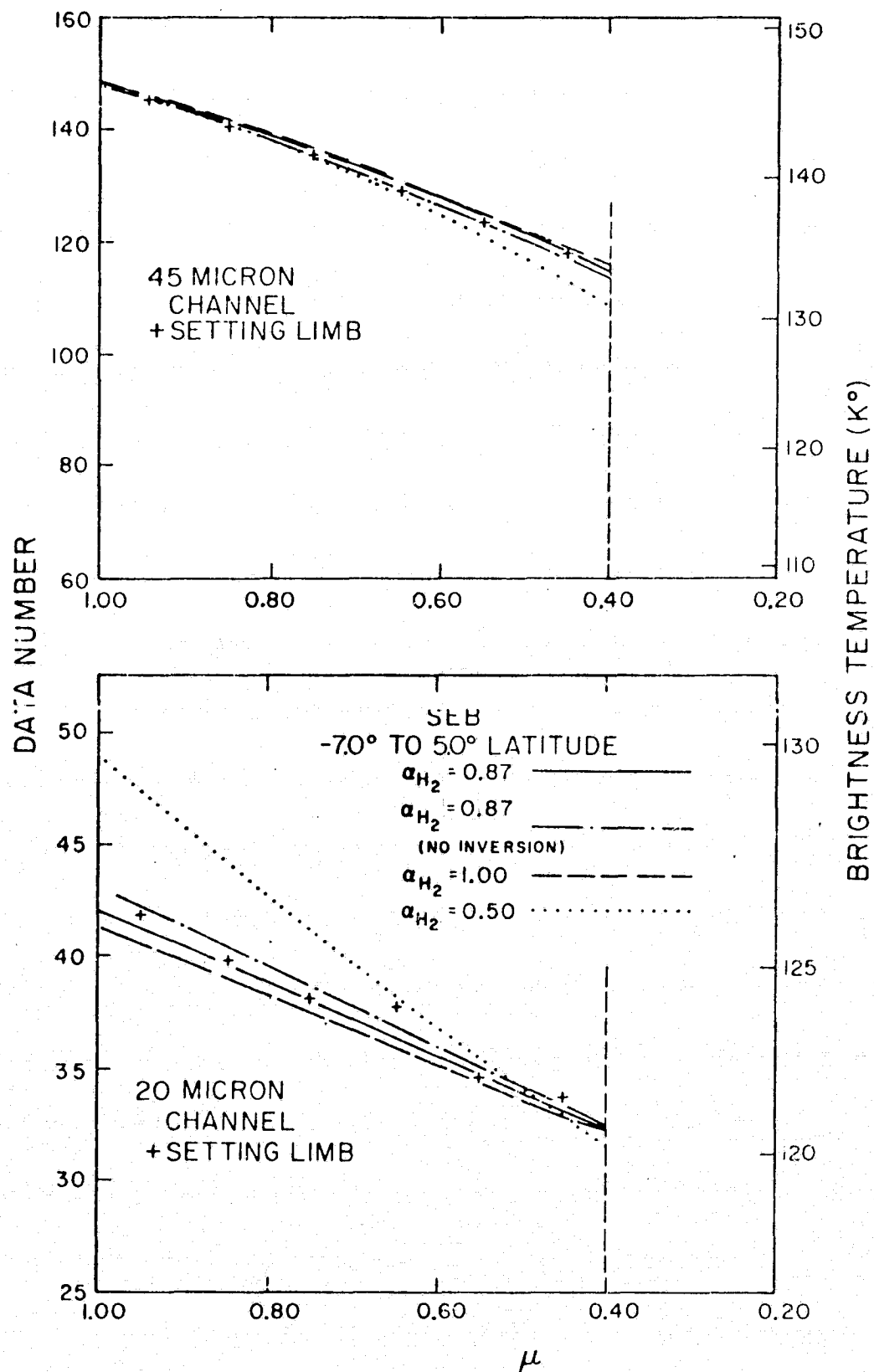


Figure D-2

altitude. Remote sensing, especially from an orbiter, is complementary: it gives global coverage, and is particularly valuable is supplemented by the "ground truth" of a probe. An excellent probe payload, with a mass about 30 kg, would be the following:

Table D-1. Basic Payload

Objective	Instruments
Atmospheric Structure	Pressure Gauge Thermometer Accelerometers
Composition	Mass Spectrometer Gas Chromatograph
Cloudiness	Nephelometer
Energy Balance	Net-flux Radiometer
Trapped Radiation	Counter
Upper Atmosphere	Neutral Mass Spectrometer Ion Mass Spectrometer Langmuir Probe

All of these, except the trapped-particle counter, are aboard Pioneer-Venus.

A competent payload could still be assembled for 20 kg by omitting the aeronomical instruments and reducing the scope of some of the others. We note, however, that the Jovian ionosphere has been found by Pioneers 10 and 11 to be very complicated and much hotter than expected. Its structure and energy balance are therefore of very high interest. These instruments would probably be strapped to the outside of the probe in such a way as to be released at entry, with a saving of weight and volume for the shielded vehicle.

A menu of other instruments has also been assembled in Table D-2. Breadboards of the first two instruments exist, and the shock-layer radiometer flew successfully on the Planetary Atmosphere Experiment Test. The alpha instrument is descended from one on Surveyor, but uses forward-scattering geometry to specialize it for a direct, unambiguous measurement of the ratio  $H\alpha/H_2$ .

Table D-2. Other Instruments.

Objective	Instruments
Composition	Alpha Scatter Speed of Sound Shock-layer Radiometer
Cloud Particles	Particle Size Spectrometer
Energy Balance	Multichannel Radiometer
Aeronomy, ionosphere	Ion Retarding Potential Analyzer
Trapped Radiation	Many
Magnetic Field	Magnetometer

The full report of OPEPSSG can be referred to for descriptions of the atmospheric instruments mentioned here, with analyses of their capability. Generally speaking, the situation is good; it is straightforward to adapt the instruments from Venus conditions to outer-planet conditions, and the colder environment is mostly beneficial. A few words are, however, in order about measurement of the composition, which includes many sub-objectives of varying difficulty. The situation is summarized in Table D-3, where the sensitivities shown are relative to the most abundant gas,  $H_2$ . The basic composition of the atmosphere is defined by the first two objectives, which are readily achieved by existing techniques (within the limitations of freezing-out of the volatiles in (2) in a cold atmosphere). Pumping of helium does offer some problems, but we are convinced that they are manageable. Flight mass spectrometers are generally unable to measure a mass peak to better than a fraction of a percent, which generally surpasses the limit of a feasible calibration (except for isotopic ratios). The performance of a gas chromatograph is generally similar.

Table D-3. Composition Objectives

Measurement	M. S.	G. C.
1. $He/H_2$	good	good
2. $CH_4$ , $NH_3$ , $H_2O$	good	good
3. Rare constituents	$\geq 10^{-7}$	$\sim 5 \times 10^{-6}$
HCN, $CH_3CH$ , hydrocarbons	as above	$\sim 10^{-9}$
4. Isotopes	$^3He$ , Ne, Ar C, N, O D ?	----

The instruments have a considerable capability beyond the basic measurement. The dynamic range of existing mass spectrometers is about  $10^7$ ; the gas chromatograph is typically somewhat less sensitive, with a couple of exceptions. We really do not know what gases to expect, except for the noble gases, which should be measurable as far as argon.

A final added objective is measurement of isotopic ratios. The noble gases are particularly important, and can be measured in a sample that has been passed through a getter to remove all other gases, as on Pioneer Venus. Apart from sensitivity, which probably rules out krypton and xenon, the main limitation is the fraction of a percent reading accuracy. Deuterium is a special problem, because it is rare (at least on Jupiter), and its principal peak ( $\text{HD}^+$ , mass 3) is blended with  $\text{H}_3^+$ . More work is needed to evaluate the possibilities.

## APPENDIX E

### STUDIES OF THE OUTER SOLAR SYSTEM WITH THE LST --- A WORKING PAPER\*

#### 1. Introduction

Attempts to evaluate the capabilities of the LST necessarily suffer from present uncertainties about the size of the telescope, the characteristics of the ancillary equipment, and indeed the future of the entire project. Ignoring these problems, one can evaluate the potential of various types of observations from an Earth-orbital telescope in a parametric form, so that the effects of changes in instrumentation can be evaluated at least in an idealized way. In fact, many such studies have been carried out over the last few years, and a bibliography of representative publications is appended.

#### 2. General Considerations

The great advantages of working from an Earth-orbital platform are

1. Unrestricted wavelength coverage
2. High angular resolution
3. Long time-base for observations
4. Opportunity for "directed" observations
5. Opportunity to make use of latest technology

This list appears at first glance to provide a capability for precisely the kind of observations we must have in order to advance our present meager knowledge of the outer solar system by a significantly large factor. The first two items represent advantages over ground-based observations, the last three are advantages over observations from space-craft. This mixture is typical of the LST, which offers a blend of some of the most powerful ground-based and space borne techniques. The principal disadvantages compared with space missions are

1. Limited angular resolution
2. Limited phase-angle coverage

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\*T. Owen: January 10, 1975; Revised March 10, 1975.

The way in which these assets and liabilities affect the kinds of observations one would like to make can be judged from the following sections.

### 3. High Resolution Imaging

The angular resolution for two classes of LST, 3-meter and 1.5-meter, is given in Table E-1. We have accepted the criterion that the practical limit for objects of moderate contrast is twice the theoretical diffraction limit (Danielson 1974).  $D$  is the diameter of the object involved and  $L$  is the linear dimension that can be resolved at the distance of the object, accepting the above criterion. The importance of large aperture is readily apparent from inspection of the table.

A representative list of useful observations of the outer planets and their satellites that could be carried out with the 3-M LST and a multi-spectral imaging instrument is given below. The degree to which the goals implicit in this list are achievable will depend on the resolution and wavelength coverage that the actual LST can deliver.

1. Measurement of Uncertain Diameters  
(Pluto, Triton, Callisto, etc.)
2. Determination of rotation rates (including latitude dependences) for Uranus and Neptune, spectroscopically or by inspection (see #5).
3. Search for surface features on resolvable satellites (Polar caps, albedo variations, rotation rates, spin axis orientation)
4. Limb darkening curves at different wavelengths for atmospheric and surface structure
5. Search for clouds and studies of general circulation (Saturn, Titan, Uranus, Neptune)
6. Synoptic meteorology and studies of weather systems (Jupiter)
7. Chromophore production, transport, and identification (Jupiter, Saturn, Titan, Io)



Table E-1. Angular Resolution of the LST\*

Object	D	3 M - LST		1.5 M - LST	
		L	D/L	L	D/L
JUPITER	137,400	200	690	400	345
J V	(220)		1		---
Io	3640		18		9
Europa	3100		15		7
Ganymede	5270		26		13
Callisto	5000		25		12
J VI	(160)		--		--
SATURN	115,100	420	270	840	135
Janus	(370)		--		--
Mimas	(400)		1		--
Enceladus	(500)		1		--
Tethys	(1000)		2		1
Dione	1150		3		1
Rhea	1600		4		2
Titan	5800		14		7
Hyperion	(350)		--		--
Iapetus	1520		4		2
Phoebe	(260)		--		--
URANUS	50,100	900	56	1800	28
Miranda	(400)		--		--
Ariel	(1400)		1		--
Umbriel	(1000)		1		--
Titania	(1800)		2		1
Oberon	(1600)		1		--
NEPTUNE	49,400	1400	35	2800	17
Triton	(4000)		3		1
Nereid	(600)		--		--
PLUTO	≤6400	1600	≤4		≤2

\* Linear dimensions in km

8. Transient phenomena (Post-eclipse brightening of satellites,  
Io sodium cloud, hydrogen halos at Jupiter-Io and Saturn-Titan)

#### 4. Spectroscopy

Low resolution IR observations from the LST will easily be able to determine whether or not Uranus and Neptune have internal energy sources. However, this is one of many questions that we now have which may be answered by other types of observations before the LST is launched. The infrared region of the spectrum is vulnerable to attack by low resolution airborne observations coupled with high resolution ground-based studies. This is a very powerful combination which seems bound to deliver more and more information as instrumental sophistication increases during the next few years. The LST can help significantly by allowing observations to be made with higher angular resolution, but this advantage is more likely to be realized at shorter wavelengths.

Center-to-limb studies of the type that are presently being carried out on Jupiter should become practical on all the planets except Pluto. This would supplement the program implied by item 4 in the previous section.

Studies of the ultra-violet spectral region with high angular resolution could be very helpful in delineating atmospheric structure on the major planets and in searching for minor constituents — including organic molecules and sulfur compounds — in all of the bodies with atmospheres.

Present ideas about the surface chemistry of the satellites and the composition of the rings of Saturn should be sharpened by extending the wavelength range of the observations (especially toward the UV) and by permitting some delineation of major terrain units that may show compositional differences. This has proved to be a very useful type of analysis for both the moon and Mars.

Spectroscopic studies of special areas on the planets may also prove very productive. This is especially important for efforts to identify

chromophores, since colored regions on Jupiter are often of very limited size, and ground-based observations are seldom confined to well-defined regions on the planet. The same is true for the equatorial region on Saturn. There is the additional intriguing possibility of obtaining multi-spectral observations of the 5-micron "hot spots" on Jupiter, thereby further delineating the nature of these phenomena while looking for minor atmospheric constituents revealed by the long path length into the Jovian atmosphere at this wavelength. The 2.7-micron window in the outer planet atmospheres would become accessible for the first time from this observatory.

## 5. Some Conclusions and Reservations

It is clear that the LST could make major contributions to our understanding of the outer solar system if it is realized in its most advanced form and if large amounts of observing time are made available for planetary programs. Since neither of these conditions is likely to exist in practice, the gain in information will be less spectacular than the preceding discussion might lead one to expect. We must also remember that some of the questions we want to answer may be resolved by ground-based and air-borne observers long before an LST becomes operational.

To try to evaluate the relative merits of the LST and planetary missions, it is helpful to consider some major problems that are unlikely to be solved by Earth-orbital or ground-based observations, and to weigh these in importance against the problems that can be solved. A sample list follows:

1. The hydrogen to helium ratio in the atmospheres of the major planets, including isotopic abundances.
2. The abundance and isotopic composition of neon in the atmospheres of the major planets and Titan.
3. The pressure and temperature profiles in the atmospheres.
4. The identification of the major component(s) of the atmosphere of Titan.

5. The identification of the colored material on Titan, Jupiter, Io, Saturn.
6. The location and composition of cloud layers in the atmospheres of the major planets and Titan.
7. A definitive search for spectroscopically inert atmospheres on Pluto and Triton.

Most of these questions can be attacked from the ground or from an LST, but it is doubtful that they can be given definitive answers. Abundances of helium, neon, argon, and nitrogen seem particularly crucial for an understanding of the atmospheres of Uranus, Neptune and Titan, and it is hard to see how we will obtain this information without atmospheric probes. The same is apparently true for the pressure-temperature profiles, which even fly-by occultations are presently unable to deliver.

Finally, it should be realized that no mention has been made of gravitational field mapping (for internal structure), mass determinations, magnetic field and charged particle investigations, interplanetary dust studies, etc., etc. — the many additional types of investigations that demand spacecraft for their implementation.

One is therefore inclined to support the LST as a useful supplement to a program of space missions, but in no way as a substitute for such a program.

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## APPENDIX F

### EXPLORATION OF THE SATURNIAN SYSTEM\*

In comparison with missions to Jupiter and Uranus, missions to the Saturn system have no obvious competitive advantages in the study of the planet itself. Conceivably unique subjects of study in the Saturnian system are the following: (1) the particle rings; (2) the interaction of the Saturnian magnetosphere and trapped particles, if they exist, with the particle rings; and (3) the satellites, particularly Iapetus and Titan. A preliminary investigation of all three subjects will be made in the Mariner Jupiter/Saturn mission.

The most likely and the most exciting MJS follow-on mission is a Titan entry probe and soft lander. The development of such systems requires a better characterization of the Titanian environment, the most critical missing factor in which is the determination of the surface temperature and pressure. This could be obtained by a radar measurement of the diameter of the solid body of Titan. However, the existing Arecibo system has a signal to noise ratio 20 db down from what is required. Much more practical is an improvement on Briggs' direct measurement of the radio brightness temperature of Titan. This requires microwave interferometry to distinguish Titan from other faint background sources. A reduction in the existing error bars by a factor of two or three would probably be adequate to characterize a Titan entry mission. NASA should make a special effort to encourage the National Radio Astronomy Observatory and the National Astronomy and Ionosphere Center — the two institutions which have the obvious capability of performing such a measurement — to do so. This will calibrate both the scientific interest of Titan and its accessibility to entry probes.

A small but important effort in the exploration of the Saturnian system can be performed by optimizing the Mariner Jupiter/Saturn mission toward the exploration of Saturn. In the next few decades there are

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\*C. Sagan

likely to be a number of missions directed at Jupiter, or which will use Jupiter in swingby mode for the exploration of Uranus, Neptune or Pluto. The first Mariner class mission to both Jupiter and Saturn should weight heavily the exploration of the Saturnian system. Unfortunately because of the greater distance of Saturn, the data rate from Saturn on MJS will, in the present mission configuration, be lower by about a factor of 5 than it is at Jupiter. For example, the total number of high resolution photographs (of the order of several kilometers resolution) from both Mariners 11 and 12 will be no more than about 20. No three-color global motion pictures for cloud motions and chromophore studies will be made during late encounter phase for Saturn, as they will for Jupiter. The color, narrow-band filter, mosaicing and variable features coverage of the Saturnian satellites will likewise be badly compromised. The limited number of pictures of Saturn also severely impedes the important objective of comparative meteorological studies of Jupiter and Saturn from the MJS mission.

This problem can be substantially relieved by an upgrading in DSN capabilities. There seems little prospect that the Arecibo telescope could be resurfaced for X-band by 1981 and there seems little prospect for improvement in the X-band transmitter power aboard MJS. However a proposal to erect a duplicate 210 foot dish at Goldstone, and perhaps the two other DSN stations, could, if implemented, double the data rate from Saturn. This is in effect the equivalent of launching two more Mariner class spacecraft to Saturn but at less than a few percent of the cost. There are of course a large number of other advantages for planetary exploration by such an upgrading of the DSN.